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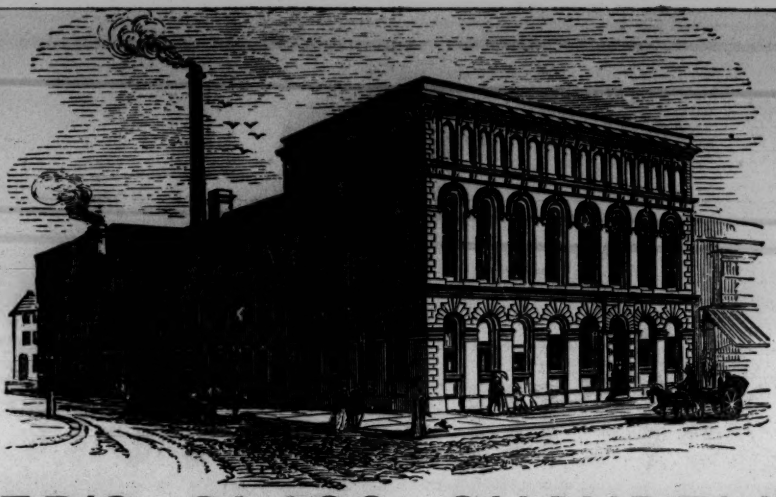

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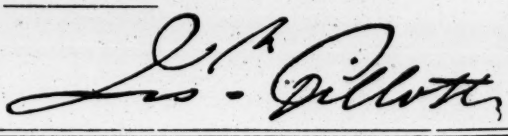

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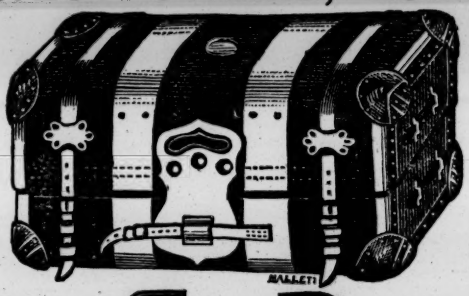

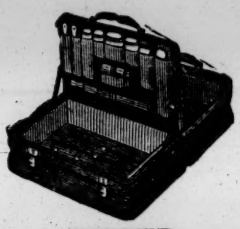

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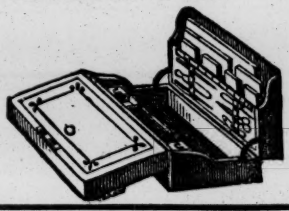

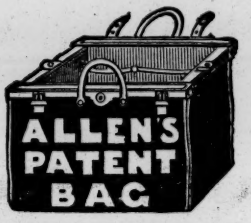

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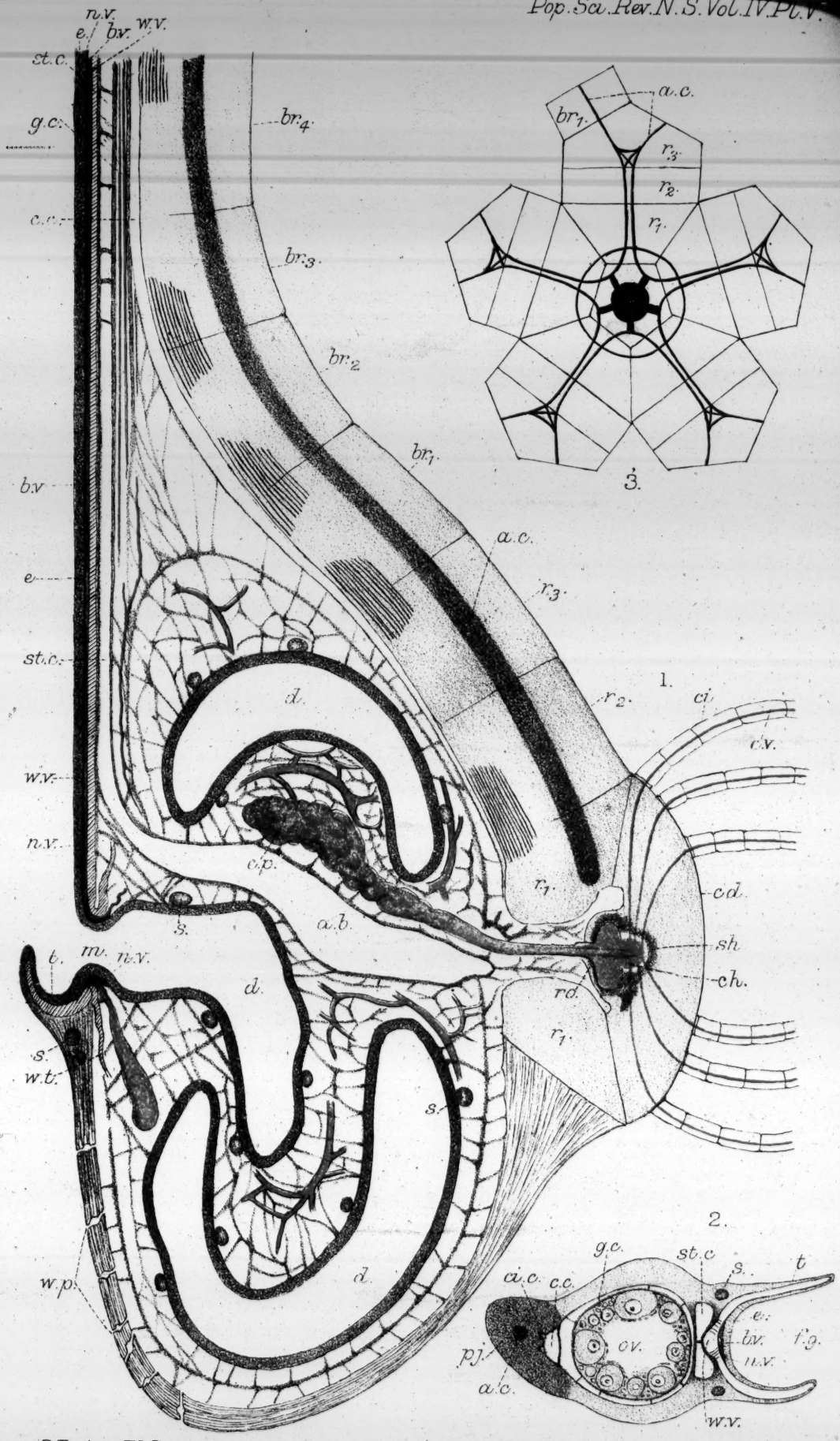

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COMATULÆ.

FEATHER-STARS, RECENT AND FOSSIL.

By P. HERBERT CARPENTER, M.A.,
Assistant Master at Eton College.

[PLATES V. AND VI.]

THE *Comatulæ*, or Feather-Stars, have long been known as among the most beautiful and interesting of the inhabitants of the shallow water round our coasts. They are found in all parts of the world, and at all depths down to 2900 fathoms. The Arctic Expedition of 1875–76 dredged two species in Kennedy Channel, above 80° N. Lat. One at least of these is common in the seas of Spitzbergen and Nova Zembla, while a closely allied form was obtained by the *Challenger* at Heard Island in the Southern Sea. They are largest and most varied in the tropics, especially in the shallow water about the Philippine Islands and in the Malay Archipelago. At great depths they are much dwarfed, and at the same time somewhat rare, the *Challenger* having met with them but seven times at depths exceeding 1000 fathoms.

The *Comatulæ* belong to the *Crinoidea*, which is a very well-defined class of the great sub-kingdom Echinodermata, differing from the other members of the group, *e. g.*, the Starfishes, Sea-Urchins, and Sea-Cucumbers, by various peculiarities.*

While the Starfishes and Urchins crawl about mouth downwards on the sea-bed, by the aid of numerous sucking feet, the Crinoids remain more or less fixed in one spot, lying on their backs or growing on stalks, with the mouth upwards. Their arms which may be from five to about two hundred in number, are fringed by small appendages, the pinnules (Pl. VI. figs. 1, 2, *pi.*), which alternate on opposite sides of each arm, and give rise to the appearance which is denoted by the name Feather-Star. The arms may be more or less completely extended, or one or more of them may be curved inwards over the mouth; but

* Compare 'Notes on the Ophiurans, or the Sand and Brittle Stars,' by Prof. Duncan, F.R.S., *Pop. Sci. Review*, N. S. vol. ii. (1878) pp. 339, 342.

they have never been seen to take part in the prehension of food. It is, however, indirectly obtained by means of the arms.

On the upper surface of each arm and pinnule there is (in most cases) a narrow groove (Pl. V. fig. 2, *f.g.*, and Pl. VI. fig. 1, *f.g.*), which is lined by a number of those delicate little protoplasmic filaments, known to naturalists as cilia. These are in a state of continual vibratory movement, which is always in the same direction, viz. towards the mouth. Currents are thus set up in these food-grooves, all making for the mouth, to which the grooves converge. Any of the small alimentary particles scattered in the water which may happen to settle down in one of these food-grooves are thus carried along it towards the mouth. The grooves of adjacent arms unite in succession so as to form from five to ten primary groove-trunks, which end in a peristomial area of variable size and shape. This may be either almost in the centre of the body (Pl. V. fig. 1 and Pl. VI. fig. 12), or altogether eccentric (Pl. VI. fig. 11). Within this peristomial area is a narrow slit, the mouth (*m*), into which are poured the contents of the various food-grooves. This, though mostly microscopic, appears to vary very much in character, according to circumstances. Dr. Carpenter has found the stomach of the Arran *Comatulæ* to contain large quantities of the horny remains of infusorial animalcules, while in other specimens the horny casings of Entomostraca, or of the larvæ of higher Crustacea, have been found in the stomach. I have myself removed from the stomach of a large tropical Comatula the body of an Isopod Crustacean nearly half an inch long, while in another case I have found the food-grooves to contain numerous foraminiferal shells, *Rotaliæ*, *Biloculinæ*, and others.

The whole of the coiled digestive tube is lodged in the body of the *Comatula*, no part of it extending into the arms (Pl. V. fig. 1, *d.*) This body consists of two parts, (1) the cup or calyx formed by the skeleton, and (2) the visceral mass, usually termed the disc, which is supported in this cup, but is sometimes very readily detached from it, as has been the case with the discs represented in Pl. VI. figs. 11, 12. The disc is usually more or less hemispherical in shape (Pl. V. fig. 1), the lower convex portion fitting into the cup, while on the flattened upper surface (Pl. VI. figs. 11, 12) is the mouth, with the food-grooves converging towards it, and a tubular projection, at the end of which is the second opening of the digestive canal.* When the mouth is central, as in the genus *Antedon* (Pl. VI. fig. 12), the anal tube (*a.t.*) is in one of the interradial spaces on the disc between two of the food-grooves; but when the mouth is eccentric, as in

* I have one monstrosity, an *Actinometra*, with two mouths and two anal tubes.

Actinometra (Pl. VI. fig. 11), the anal tube is nearly or quite central.

In our English *Comatulæ* the disc is almost or entirely bare; but in many tropical forms it is covered by a very complete mosaic pavement of closely fitting plates (Pl. VI. fig. 12), which occupy all the spaces between the food-grooves, and even extend out for some distance on to the upper surface of the arms and pinnules at the sides of their median grooves.* Most of the plates nearest the grooves are pierced by minute holes (*w.p.*), the 'water-pores.' These are the upper openings of tiny funnel-shaped tubes, which open below into the body cavity, and are lined by cilia all working inwards, so that the body cavity is in free communication with the external water. These pores are likewise present in *Comatulæ* with naked discs (Pl. V. fig. 1. *w.p.*); and they have also been found on the lowest parts of the arms, and even piercing the plating of the pinnules, where they lead into a tubular extension of the body cavity into the arm, containing the generative gland.

On the naked discs (Pl. VI. fig. 11) each side of the food-groove is formed of an elevated fold of skin scolloped at its edge so as to form a row of minute triangular leaflets. At the base of each of these is a group of three delicate tubular tentacles, one of which is much longer than the other two. The leaflets alternate on opposite sides of the groove, and are ordinarily erected, with the tentacles projecting considerably beyond them. But the tentacles may be withdrawn and the two folds closed down, the leaflets meeting in a sinuous line, so as completely to cover the furrow. The same is the case with the grooves of the arms and of the pinnules borne by them, as is seen in Pl. VI. fig. 1, while some arms are occasionally altogether ungrooved. (Pl. VI. fig. 2.)

In the plated tropical *Comatulæ* matters are much more complicated (Pl. VI. fig. 10.) The edges of the groove are supported by a series of more or less oblong side-plates (*adambulacral*), and hinged to the upper side of each of these is an oval covering-plate† (*superambulacral*). These may be erected and the tentacles extended, or they may be closed down so as completely

* The ventral plating of the disc is so very complete in some recent *Comatulæ*, that no mouth is visible at all in the dry state. The summit-plates of many Palæozoic Crinoids, such as *Cyathocrinus*, and also of the Blastoids appear to me to be strictly comparable to those of these recent forms. Hambach, however, speaks of the summit-plates of the Blastoids as non-existent or rather as proving on close examination, to be Bryozoa, or ovulum-like bodies!—*Trans. St. Louis Acad. Science*, vol. iv. No. 1, p. 150.

† Some recent Crinoids have covering-plates only, and no side-plates. The same variability occurs in the Palæozoic Crinoids, and perhaps also in Blastoids. Some species have both series represented, though, of course, in a greatly reduced form, while others have the covering-plates only. Hambach's description of the zigzag-plated integument of the Blastoids, as 'probably of an elastic texture during the lifetime of the animal,' professedly

to cover the groove, the succeeding ones from opposite sides overlapping each other, while the tentacles are retracted. The holes, (*s*) represented in fig. 10 between the adjacent side-plates mark the positions of the dark spots or 'sacculi,' which occur at the sides of the food-grooves of nearly every *Antedon*, and are also to be found in the interior of the body. (Pl. V. fig. 1. *s.*)

These tentacles of the Feather-stars correspond to the tube-feet of a Starfish-arm, and contain side branches of a tube which is situated in the middle line of the arm beneath the food-groove, and is known as the water-vessel, or ambulacral vessel (Pl. V. figs. 1, 2, *w.v.*) All the water-vessels of the different arms unite in succession as the food-grooves do, and converge to join the water-vascular ring, an annular tube, situated in the lip around the mouth.* From this ring a number of delicate tubules—the 'water-tubes' (*w. t.*)—hang down into the body cavity, into which they open. It contains water which has entered it by the water-pores on the disc, so that the ambulacral system is indirectly in communication with the water in which the animal lives. We are not yet quite clear about the real nature of the water-vascular system of the Echinoderms; but it seems tolerably certain that it forms an important part of the breathing apparatus of the Crinoids, and that it contains some oxygen-carrying substance which is able, in the thin-walled tentacles, to exchange carbonic acid for the oxygen of the surrounding water.

Superficial to the water-vascular tube of each arm is a smaller tube, the radial blood-vessel (*b. v.*) which is connected like its fellows with a blood-vascular ring around the mouth. Situated in the vertical axis of the disc is a lobulated organ known as the central plexus, from its consisting of a bundle of blood-vessels. (Pl. V. fig. 1, *c.p.*) Some of these terminate above in the oral blood-vascular ring, while others extend outwards into the rays and arms and surround the genital glands. Others again give off side branches which form a network over the digestive tube. Towards the bottom of the visceral disc the vessels of the central plexus group themselves into an inner set, surrounded by five outer ones, and so descend into the calyx. They pass through the central funnel between the inner ends of the first radials, at the bottom of which the five outer vessels corresponding in position with the radials expand considerably so as to form the 'chambered organ.' This consists of a central axis of minute vessels with five chambers (Pl. V. fig. 1, *ch.*) clustered round it like the carpels of an orange. It is contained in a small, more or less basin-shaped plate, known as the centrodorsal piece. (Pl. V. fig. 1.

refers to these plates. What he does describe, however, is something entirely different.

* Compare Duncan, *loc. cit.* pp. 347–351.

Pl. VI. figs. 7, 9, *cd.*) This forms the dorsal or lower pole of the body and bears on its exterior a number of jointed appendages, the cirri (*ci.*) by which the animal anchors itself. Each cirrus is pierced by a central canal lodging a blood-vessel (*c. v.*) which is continuous through the wall of the centrodorsal with one of the chambers of the chambered organ, or with one of the vessels in its central axis.

Soldered on to the centrodorsal are the five first radials (Pl. V. fig. 1, Pl. VI. figs. 7, 9, r_1.) forming the lower part of the cup in which the disc rests. Jointed on to them and attached to them by muscles are the five second radials (r_2.). Each of these in its turn bears a third or axillary radial (r_3.), the outer face of which is not flat but roof-shaped, and usually bears the lowest joints (br_1) of two arms working on it by means of muscles, as the second radials work on the first. These arms, which are thus primarily ten in number, consist of a series of joints that may be all like the first. But in other cases some of the joints may be axillary, so that the primary arms fork as the rays do. In the deep-sea *Comatulæ*, in the two British species, and in those from cold climates, the ten primary arms rarely divide, but in the tropical species the forking of the rays may be so often repeated that the number of arms becomes very great, sometimes reaching nearly two hundred. Each arm-joint (with a few exceptions) bears a similarly jointed appendage, the pinnule (Pl. VI. figs. 1, 2, *pi*), which is merely a small edition of the arm, containing prolongations of the water-vessel, the blood-vessel, the body-cavity, and in the case of the lower pinnules of the arm, the genital glands also (Pl. V. fig. 2).

Each joint of the rays, arms, and pinnules, is pierced by a central canal lodging a fibrillar cord (Pl. VI. figs. 1, 2, *a. c.*), that proceeds through the first radials from a yellowish fibrillar envelope (*sh.*) around the chambered organ. A sheath of the same substance surrounds each of the cirrus vessels (*c. v.*) that proceed downwards and outwards from the chambered organ.

From each of the interradial angles of the chambered organ five large cords pass upwards and outwards, and fork almost immediately. (Pl. V. figs. 1, 3.) The right branch of one fork and the left branch of its neighbour enter two adjacent openings on the inner face of each first radial. They run side by side through its central canal and on into the third radial where each of them forks. The two right branches enter the central canal of the skeleton of the right arm, while the left branches enter that of the left arm to form their respective axial cords. Before leaving the third radial, however, these two cords are united by a transverse commissure. There are also commissures in the first radials. The two cords which each contains are united with one another and with those of adjacent radials by

one continuous circular commissure lodged like the other cords in special canals.

What is the meaning of this excessively complicated arrangement? Experiment shows that the movements of the arms are dependent upon the integrity of their axial cords and upon the connection of these cords with the central fibrillar envelope of the chambered organ. The swimming movements of a Feather-Star are exceedingly active, and are also performed with a singular regularity. When a ten-armed animal swims, all the five right arms are simultaneously bent, and then the five left arms. As long as the swimming lasts this alternating movement is kept up with the most perfect regularity. Owing to the length of the arms and to the small size of their component joints, of which there are frequently more than one hundred to each arm, the number of muscles concerned in the movement reaches at least one thousand pairs.

Experiment shows that these muscles are under the influence of a governing centre, which not only regulates their contractions, but co-ordinates these contractions in the most remarkable manner. This co-ordination is well shown in the following experiment. When one of the first pair of pinnules on the arm is irritated, the whole circlet of arms is suddenly and simultaneously closed over the disc; but irritation of one of the ordinary pinnules higher up the arm is simply followed by flexion of the arm which bears it. The governing centre on which this action depends has been shown to be situated in the fibrillar sheath of the chambered organ; and the axial cords of the rays and arms are the channels by which the influence of the centre is communicated to the muscles. For the swimming movements of the whole animal depend upon the integrity of the chambered organ, and may take place after the visceral disc has been removed from the skeleton, which contains the chambered organ and axial cords, and swims about on its own account. The swimming movements are therefore entirely independent of any structures contained in the disc. In the same way the movements of each individual arm depend upon the integrity of the axial cord of that arm, stopping directly it is injured; and microscopic investigation shows that branches of the axial cord are distributed upon the ends of the muscular bundles which connect the successive joints of the rays, arms, and sometimes also of the pinnules.

The above facts seem to show that the axial cords of the rays and arms, together with the fibrillar sheath of the chambered organ in which they originate, constitute a system of motor nerves of no little complexity. The only difficulty in the way of this view is that nothing of the kind is known in the other Echinoderms. The Star-fishes, for example, have a fibrillar band between the radial blood-vessel and the cellular lining of

the median groove of each arm, which is connected with an oral ring. This ring, and the bands connected with it, constitute a nervous system situated on the ventral side of the body.* A similar oral ring with radial prolongations occurs also in the Feather-Stars (Pl. V. figs. 1, 2, *n. v.*). But it is altogether unconnected with the muscles, and has no influence whatever on the swimming movements. After the visceral mass has been turned out of the calyx altogether these movements go on just as well as before, while the movements of any arm are entirely independent of the ventral nerve of that arm; for the ungrooved arms (Pl. VI. fig. 2), with no ventral nerve, swim just as well as the grooved arms with a ventral nerve, even after the latter has been divided. In some tropical *Comatulæ*, half, or more than half, of the arms are in this ungrooved condition.

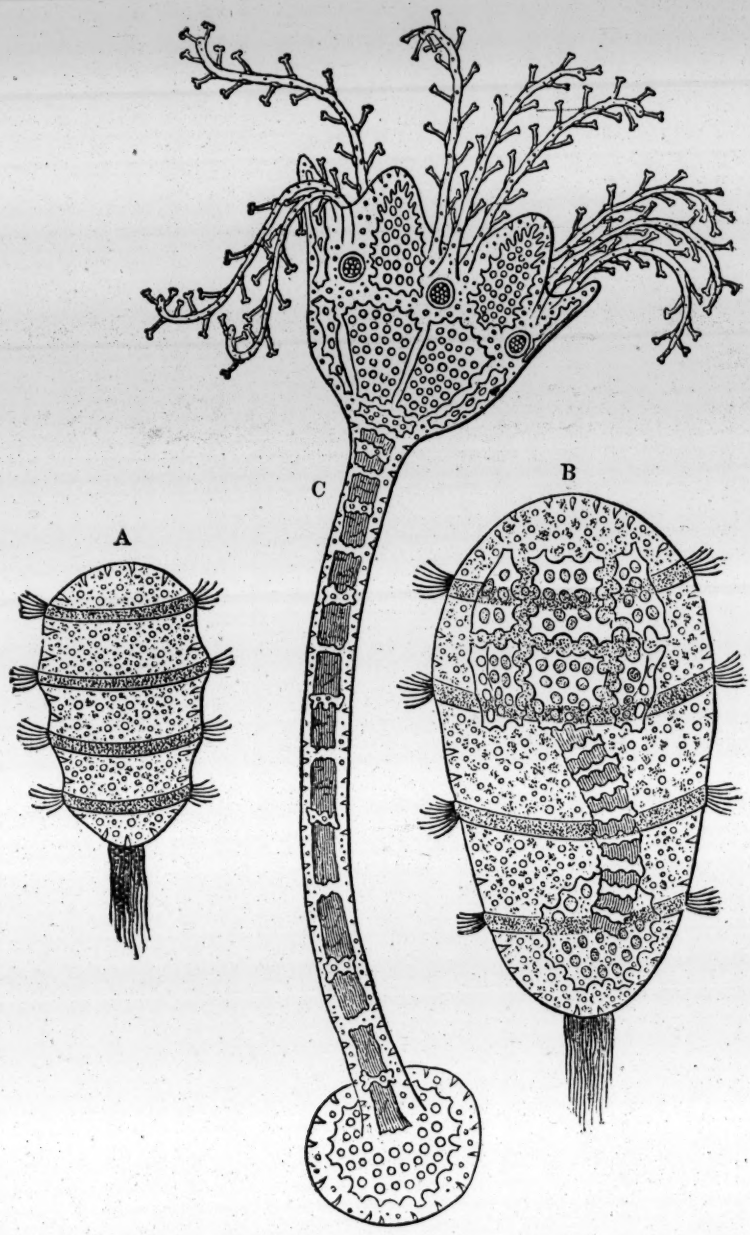
While on the one hand the nervous nature of the ventral fibrillar band can hardly be denied, owing to its structural identity with the nerves of other Echinoderms, we must admit, on the other hand, that the very considerable locomotive powers of the *Comatulæ* are localized in the chambered organ and its connections. These are altogether unrepresented in the other Echinoderms, but in spite of this, and of the singularity of their position, we cannot, with justice, refuse to consider them as nerves. Some day, perhaps, their presence will be understood.

Between the dorsal skeleton of the arms and pinnules, and the water-vessels on their ventral side, are three tubular prolongations of the body-cavity. The middle one is known as the genital canal (Pl. V. figs. 1, 2, *g, c.*), because it contains the genital gland (*ov.*) surrounded by a blood-vascular space, which is connected with the vessels of the central plexus. The canals above and below this one, are known respectively as the ventral or sub-tentacular (*st. c.*) and the dorsal or cœliac (*c. c.*). They communicate with one another at the end of each arm or pinnule, and are connected in the body with different parts of the body-cavity, which is full of water containing the products of digestion. The sub-tentacular canal arises from a large central space (*a. b.*) in the axis of the body, around which the digestive canal is coiled, and a current proceeds through it to the tip of each arm and pinnule, which returns to the body by the cœliac canal. This current is produced by the vibration of cilia, which are not uniformly distributed, but localized in little cups on the top of each pinnule-joint (Pl. V. fig. 2, *ci. c.*).

The young of the Feather-Star leaves the egg as a little oval body about $\frac{1}{30}''$ in length, shaped somewhat like a small barrel, and surrounded by four hoops of long vibratile cilia with a still longer tuft of them at its hinder end (woodcut A). By means of these cilia it swims about in the water. After a while, slender limestone rods make their appearance near the front

* Compare Duncan, *loc. cit.* pp. 348–351.

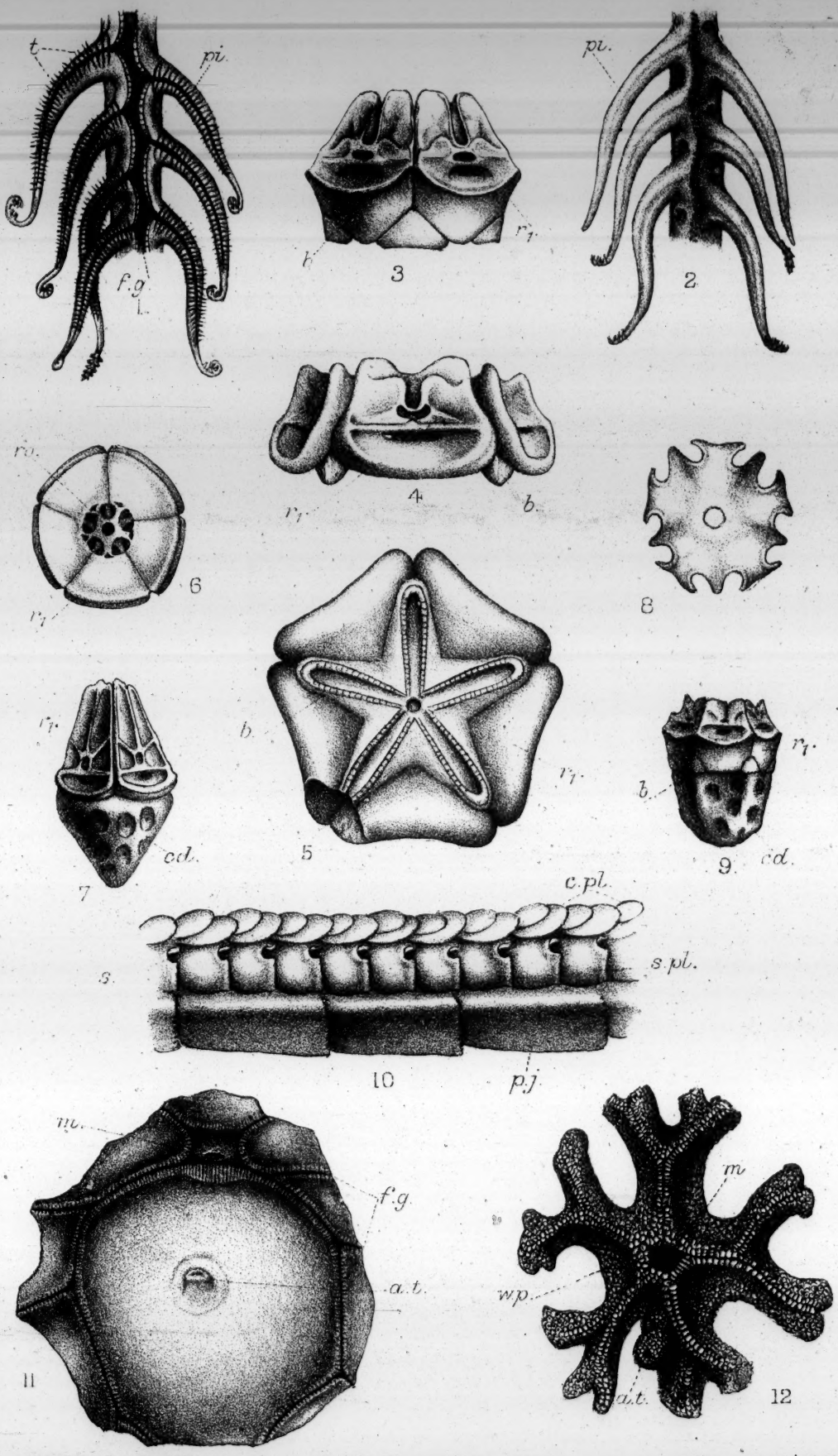
end, and by repeated forking and joining these rods give rise to ten plates of a delicate calcareous network, which are arranged in two cross rings of five plates each (woodcut B). Passing back-



Three stages in the development of *Antedon rosacea* (after Sir Wyville Thomson).* A. Larva just hatched; B. Larva with rudiments of the stem-joints; C. Pentacrinoid larva.

wards from beneath the centre of the lower ring of plates is a series of delicate calcareous rings, which become supported, later on, by bundles of longitudinal rods forming inside them. The last

* We are indebted to the courtesy of Sir John Lubbock for permission to use this woodcut.



C. Berjeau F.L.S. del et lith.

Mintern Bros. imp.

COMATULÆ.

of these bundles, at the hinder end of the larva, rests against a circular plate of considerable relative size. At this stage the larva has the form of a bent club or rod with an enlarged head. The ciliated bands disappear, and it gradually loses its power of swimming, attaching itself to some stone or other solid substance by its base, the knob of the club being free (woodcut c). This knob gradually becomes the body of the future *Comatula;* while the series of rings between it and the base of attachment make up a short supporting stem. This is known as the Pentacrinoid stage of development, owing to the resemblance between it and another Crinoid (*Pentacrinus*) which remains stalked throughout life, while the larval *Comatula* is only so for a time. For it eventually separates itself from all but the top joint of its stem, and anchors itself by the cirri that are developed upon this top joint, which developes into the permanent centrodorsal piece, being rather larger than the other stem-joints from the very first. The ring of plates resting upon it are the basals, and the plates above them, the orals. The enlarged head gradually becomes five-lobed, each lobe answering to an oral plate; and after a while these plates separate like the petals of a flower so as to expose in the centre (*Antedon*) the opening of the permanent mouth. Around it are five groups of three tentacles each, alternating with the oral-lobes. At this early stage no radials are present, but only the basal-plates resting upon the centrodorsal piece beneath, and supporting the orals above. The space between these two rings gradually increases, the orals being carried away from the basals as the calyx widens out. The first radials appear in this intervening space between the basals and orals, alternating in position with both; and in correspondence with them the rays grow out as rapidly-elongating processes, in which the other radials are successively developed, so that the diameter of the calyx increases fast. The oral plates retain their primitive relation to the mouth, and so get carried further and further away from the basals, while the first cirri begin to appear on the centrodorsal ring that supports the basals. The orals gradually become absorbed, and soon after the head detaches itself from the stem and commences life as a free *Comatula* they disappear altogether. At the same time the basals undergo a very singular series of changes which result in their being no longer visible on the exterior of the calyx.

Very soon after the young *Comatula* is detached from its stem, the centrodorsal begins to extend itself over the dorsal surface of the ring of basals so as finally to conceal it altogether. At the same time the shape of these plates becomes altered by removal of one part and the deposition of new calcareous matter at another part. The original ring of five pentagonal plates thus undergoes transformation into the 'rosette,' a small ten-

rayed circular plate (Pl. VI. fig. 8) which rests on the middle of the upper surface of the centrodorsal piece so as to cover in the chambered organ above. (Pl. V. fig. 1, *ro.*) It is lodged in the lower part of the funnel formed by the first radials, which now rest directly on the centrodorsal, and it becomes more or less closely united to their inner ends, so that when the circlet of radials is separated from the centrodorsal, the rosette comes away with it. This is shown in Pl. VI. fig. 6, representing the under surface of the radials of *Antedon celtica* with the rosette (*ro*) in the centre of the ring formed by them.

This metamorphosis of the basals of recent *Comatulæ* into a concealed rosette is a very singular circumstance, because it shows that the *Comatulæ* have, as it were, taken a new departure, and have branched off from the type of *Pentacrinus* in the resemblance of which they have developed. In all the recent *Pentacrinus* species and in most of the fossil ones the radials are cut off from the top stem-joint by a more or less complete ring of basals. (Pl. VI. fig. 3, *b.*) These basals are sometimes small, and only just in contact by their central ends, while their outer ends appear externally as small buttons separating the radials from the top stem-joint at the angles of the calyx. There is every gradation between this condition and that in which they meet one another all round, so that the radials rest entirely upon them, and are nowhere in contact with the stem-joint; but we know of no recent *Pentacrinus* in which the basals do not appear externally, though some of the fossil forms are in this condition. On the other hand we have no certain knowledge of any fossil *Comatulæ* with a rosette. The earliest known *Comatulæ* occur in the Inferior Oolite of this country. Figs. 4 and 5 on Pl. VI. are two views of the calyx of one of them, *Ant. cheltonensis.* The difference between them and figs. 6 and 7, which represent the corresponding parts of a recent species, is at once apparent. In the fossil the basals form a five-pointed star, lying beneath the under-surface of the radials (fig. 5); while in the recent species all that remains of them is the inconspicuous rosette occupying the gap between the inner ends of the radials (fig. 6).

The Oolitic rocks of South Germany and of Switzerland, which are of approximately the same geological age as the Oxford Clay and Coralline Oolite of the British area, contain several different kinds of *Comatulæ*, far more than are known in this country. Fig. 9 on Pl. VI. represents the calyx of one of them, *Ant. scrobiculata.*

All the Oolitic *Comatulæ* have basal pieces like those of this species and of *Ant. cheltonensis* (figs. 4, 5), though they are not always so large. In some forms they do not appear at all on the outside of the calyx, so that they are only visible in specimens from which the centrodorsal has been removed. This is

especially marked in the Cretaceous species; for while some Oolitic forms have really large external basals, this is not the case in any Cretaceous species, while they are occasionally absent altogether. As, however, we are unable in these cases to see the interior of the calyx, it is impossible to determine whether a rosette is present or not. It is the same with the only two Tertiary species the calices of which are known to us.

We cannot, therefore, say with certainty when the *Comatulæ* first ceased altogether to retain their larval basals on the exterior of the calyx, as *Pentacrinus* species do still (though some of them formerly did not), and as most of the Oolitic *Comatulæ* did.

It is certain, however, that the final change occurred after the middle of the Cretaceous period; for *Antedon Lundgreni*, from the Upper Chalk, had *Pentacrinus*-like basals appearing externally. The disappearance of the basals from the exterior of the calyx was not completed, therefore, even at this comparatively recent date; and it is just possible that a very remarkable species, now living (?) in the Indian Ocean, may be the last survivor of this more generalized form of *Comatula*, with *Pentacrinus*-like basals and no rosette. Unfortunately but one example of this species has ever been discovered. Fifty years ago it was dissected, and a description was written of it; but it is not complete enough to enable us to settle this interesting question. At any rate, none of the deep-sea *Comatulæ* brought home by the *Challenger* have any resemblance to the older species, every one of them having a rosette.

Other questions which naturally arise are the following: For what reason did the basals of *Comatulæ* cease to be five more or less separate pieces and undergo transformation into the rosette; and why is it that *Pentacrinus* and *Comatula* have varied in different directions? All recent *Pentacrinus* species have external basals, but some fossils have not. Some fossil *Comatulæ* have them and others have not; but they do not appear in any recent forms (with one possible exception), for the larval basals never become prisms or wedges the outer ends of which remain external like those of *Pentacrinus*, but they disappear altogether into the interior of the calyx. These questions must remain unanswered, at any rate, for the present, if not permanently.

It may be pointed out, in conclusion, that in the class Crinoidea, as in so many other groups of animals, the recent forms are the most highly specialized; and the further we go back in geological time, the more nearly (on the whole) do the *Comatulæ* approach *Pentacrinus* and other stalked Crinoids to which they have so much resemblance in their early stages of development.

DESCRIPTION OF THE PLATES.

The following letters denote the same parts throughout all the plates.

a. b. Axial body cavity. *a. c.* Axial cords of the skeleton. *a. t.* Anal tube. *b.* Basals. br_1, br_2, br_3, &c. First, second, and third brachials, &c. *bv.* Radial blood-vessel. *c. c.* Cœliac canal. *cd.* Centrodorsal piece. *ch.* Chambered organ. *ci.* Cirrus. *ci. c.* Ciliated cups. *c. p.* Central plexus. *c. pl.* Covering-plates of food-groove. *c. v.* Cirrus-vessel. *d.* Digestive tube. *e.* Epithelial lining of food-groove. *f. g.* Food-groove. *g. c.* Genital canal. *m.* Mouth. *n. v.* Ventral nerve. *ov.* Ovary. *pi.* Pinnule. *p. j.* Pinnule-joint. r_1, r_2, r_3. First, second, and third radials. *ro.* Rosette. *s.* Sacculi. *sh.* Fibrillar sheath of chambered organ. *s. pl.* Side-plates of food-groove. *st. c.* Sub-tentacular canal. *t.* Tentacle. *w. p.* Water-pores. *w. t.* Water-tubes. *w. v.* Water-vessel.

PLATE V.

The three figures on this plate are copied, with slight variations, from Dr. H. Ludwig's 'Beiträge zur Anatomie der Crinoideen,' Morphologische Studien an Echinodermen, *Vol. I. Leipzig,* 1877–1879.

FIG. 1. Diagrammatic vertical section through the body of *Ant. rosacea.* On the right side the section passes along a ray, and on the left side it is interradial.

Explanation of the shading: black, nervous system; red, blood-vascular system; oblique shading, water-vascular system.

FIG. 2. Cross section of a pinnule of a sexually mature female *Ant. Eschrichtii.* × 50.

The exigencies of space have necessitated the reversal of this figure. The skeleton should be downwards, and the food-grove upwards, as in fig. 1.

FIG. 3. Diagram of the distribution of the axial cords within the calyx of *Comatula,* showing their origin in the fibrillar envelope of the chambered organ.

PLATE VI.

Figures 1, 2, 10 *and* 12, *by permission of the Lords Commissioners of the Treasury.*

FIG. 1. Piece of a grooved arm of a new *Actinometra* from the Philippines, seen from above. × 4.

FIG. 2. Piece of an ungrooved arm of the same specimen. × 4.

FIG. 3. Calyx of a recent *Pentacrinus,* from the side. × 4.

FIGS. 4 and 5. Side and under views of the calyx of *Antedon cheltonensis* from the Inferior Oolite of Cheltenham. × 4.

FIG. 6. Under view of the calyx of *Ant. celtica,* showing the rosette (*ro*) in the centre of the ring of first radials (r_1). × 7.

FIG. 7. Side view of the united calyx and centrodorsal piece of the same species. × 7.

FIG. 8. Rosette of *Ant. rosacea,* seen from above. × 15.

FIG. 9. Calyx of *Ant. scrobiculata,* from the White Jura of Württemberg. Copied from Goldfüss.

FIG. 10. Side view of three joints of a pinnule of a new *Antedon* from the Pacific, showing the side-plates (*s. pl.*) bounding the food-groove, and the covering-plates (*c. pl.*) which rest upon them. The dark spots between every two side-plates indicate the positions of the sacculi. × 20.

FIG. 11. Disc of *Act. solaris,* with open food-grooves. Seen from above. × 5.

FIG. 12. Plated disc of a new *Antedon* from the Pacific, with the food-grooves converted into tunnels by the folding down of the covering-plates at their sides. × $5\frac{1}{2}$.

THE PORTLAND BUILDING STONE.

By the Rev. J. F. BLAKE, M.A., F.G.S.

FROM a builder's point of view the name of Portland Stone has a very different meaning from that it would have in the mouth of a geologist. To the former it would signify the produce of certain quarries, whence the stone employed in building St. Paul's Cathedral and other edifices was obtained; while to the latter it would indicate the produce in the way of limestones more or less suitable to building of a certain period of the earth's history. The first is much the most restricted meaning; for, however great similarity geologists may find in the same formation when traced over wide areas, for practical purposes, such as the discovery of valuable building stone, it is well known, or should be, that the minor changes in a bed take place very rapidly: so that it is a rare circumstance to find stone, though on exactly the same geological horizon, having the same value in quarries a few miles apart. Ignorance of this fact is said to have been the cause of the failure in the selection of good samples of dolomite for the construction of the Houses of Parliament; the stone of one quarry was recommended, and the substitution of stone from another quarry was thought to be immaterial, the stone being geologically the same.

In the case of the Portland Stone, though so-called Portland rocks are quarried in many places and used for building, in not one of them, except at a quarry near Tisbury, is the workable stone on the same geological horizon or of the same quality as that which is so famous. By an accurate survey of all the localities in which Portland rocks have been worked, the details of which have recently been laid before the Geological Society, I am enabled to reconstruct, to a certain extent, the physical geography of the period, and to assign the various beds of workable stone to their proper position in the series.

During the latter part of that portion of time known on the Continent as the Upper Jurassic Period, in which our so-called Middle and Upper Oolites were formed, the greater part of what is now France and England was submerged beneath an open sea, into whose depths fell gradually the fine argillaceous deposits of the Oxford and Kimmeridge Clays. These were doubtless derived in large measure from the denudation of the Lias, which spread in former times far to the west of its present boundary, and whose worn edges were covered in Dorsetshire and Devonshire by the deposits of the next subsequent submergence, the Greensand and Chalk. This open Jurassic sea did not retain an unbroken tranquillity; again and again was its floor upheaved, or other disturbances took place which left their marks in the changed nature of the rocks, and these changes were not continuous over the whole area, but were limited now to one locality and now to another. Nor did they take place approximately at the same time; for while the clay was still being deposited in one place, we learn both from actually tracing the rocks and by examining their fossil contents that limestone or sand were being formed at another. To these local changes and their resulting deposits the name of episodes has been assigned; and thus we may speak of the Portland rocks as representing the later of the two episodes which in our own country affected the upper Jurassic seas. It has been the custom amongst Continental geologists to call by the same name the later of two episodes that have affected their particular country; that is, the upper mass of limestone found in the midst of their clays has been called Portland. These, however, have nothing in common with our own except the order in which they have occurred. They are all of greater antiquity; and nowhere out of England are true Portland limestones found except at Boulogne and, as is reported, in the Pays de Bray.

In the general rise of the earth's surface which closed for us the Jurassic period, the area surrounding the English Channel appears to have remained longest below the sea, and to have contained the last deposits of that era. There is something peculiar, therefore, about the circumstances of the deposition of our Portland limestone which is of interest in itself. Moreover, of all the beds which pass by that name in England amongst geologists, that which has been celebrated amongst builders is the youngest.

This will be seen by a brief account of the 'Portland Stone' as a building stone, as it is worked in the various parts of the country where it occurs, beginning at the north, and ending at the typical locality in the extreme south. There is no great thickness of these rocks at their most northerly localities,

though they cannot be said to die out. Numerous quarries are opened in them along a range of country about six miles broad, which extends from a little north of Aylesbury to a line between Brill and Thame. The best quarries are on the road between the latter town and Aylesbury; those at Stone having been worked over two hundred years, and many good buildings erected from them. There are two main blocks, the upper a softer and more uniform stone, and the lower hard, irregular, and fossiliferous, with a bed full of Trigonias at its base. In the neighbourhood of Brill a particular bed, perhaps even lower in the series, is a rather more sandy limestone of darker colour, known as the 'greys.' None of these are oolitic, or of such super-excellent quality as would ever draw general attention to them. Though of no great thickness or importance, they are the only representatives of the Portland Stone in this district, for immediately above lie thin-bedded, white, freshwater limestones usually assigned to the Purbeck series. It is interesting to see the rapid change from one condition to the other without any mark of disturbance, and difficult to conceive the exact set of circumstances that should have rendered it possible; yet occasionally one does get a glimpse of the intervening conditions, as when in a quarry is seen a great rounded excavation, in shape like the section of a river, filled with the mud of Purbeck times, and then buried and preserved by overlying deposits. In such a case one sees the very river which emptied itself elsewhere into the lake where cyprids abounded, pond-snails flourished, and insects dying dropped their tiny wings. In places, too, we even find the boundary of the area of deposit, or at least its neighbourhood, where all things seem reduced to a minimum, and formations elsewhere important lie in a few feet. Thus in a quarry at Long Crendon may be seen in not more than eight feet four formations. Below is the Portland Stone, here worked for rough building material, then a foot or two of Purbeck-limestone beds broken at the top, then the ferruginous beds of the Lower Greensand, and over all about two feet of Gault with characteristic fossils! No wonder in such a neighbourhood the Portland rock should be somewhat deficient.

The worked stone of Oxfordshire is but a continuation of the same bed in Bucks, and is of no great value. Nevertheless, at Great Hazeley are beds which have long been worked, and are of a more solid quality; at the base, and also above the best block, is a bed full of the characteristic Trigonias.

The next district in which Portland Stone is worked is near Swindon, in north Wiltshire. The same beds which we have already seen yielding an inferior kind of stone are worked for local purposes in small quarries in the neighbourhood of that

town, and in a small outlier at Bourton. But the great quarries at Swindon itself, yielding a stone of considerable importance and which has been very largely worked for houses and churches in the district, belong to a higher part of the series; in fact, they are seen overlying the other. The stone here occurs in a very curious manner. There is a considerable thickness of soft material, consisting partly of loose calcareous sands and partly of comminuted shells, with occasionally a layer of decomposed but not broken shells. In this mass are great blocks of irregular shape, lying often in the direction of some falsebedding rather than horizontally. These are indurated by so great an amount of calcareous matter, that where the sand was originally prevailing they are hard and gritty; where the shell fragments were once the main constituent they become excellent building stone; but from their irregularity, working them must be somewhat precarious. In this quarry a geologist cannot fail to be struck with the singular complexity of the succeeding deposits, which, as will be seen, must represent in time the period when the far-famed stone of the Isle of Portland might have been forming. One can see represented as in a picture many of the features of that ancient land, on which but little further to the south flourished the cycads and conifers of the Purbeck. Here one may almost see the petrifying spring issuing from the underlying limestones, by its consolidated deposits of calc-tuff; there one may trace the winding course of the ancient river, with its stone-covered base and muddy banks, its channel now filled with carbonaceous clay. Here is represented the tranquil lake, in which the calcareous matter sank to rest or was precipitated; and there, perhaps, the remnant of the soil which once covered the dry land, but was redistributed on its submergence. The teachings of this quarry are most instructive and interesting, though one unversed in field geology might need some guide, philosopher, or friend, to read them for him aright.

Out of the Isle of Portland itself, the best stone is that obtained from the Vale of Wardour, through which the South Western Railway runs between Salisbury and Templecombe. There are many large quarries about Tisbury which send stone away far out of the district, though less appears to be worked there now than there formerly was. This stone is on the same geological horizon as that which is found in the great quarry at Swindon, and with one exception all the quarries are excavated for this. It is, however, of a very superior quality. It is scarcely a limestone, but rather a calcareous freestone, for it is composed of very fine grains of sand consolidated by calcareous matter. It is sometimes obscurely false-bedded, and

comparatively free from joints. The quarries present perpendicular cut faces, retaining the marks of the instruments used for the extraction of the stone. Blocks of considerable size are extracted, and easily cut while soft into any required pattern; the colour, however, is not very good in the fresh state, being a yellowish-grey, but soon tones down on exposure to the weather into a nondescript 'stone' colour; and in this respect, therefore, the stone is inferior to the production of the island. In some quarries the goodness of the stone is interfered with, though its interest for geologists is increased by a band of chalcedony, which has congregated sometimes round Trigonias and sometimes round corals. The latter are very beautiful objects, of which specimens are scattered through many collections; but they are now apparently utterly exhausted, and no more are to be obtained. These bands have doubtless, like the flints, been formed by infiltration through the surrounding mass, but here lie in bands instead of irregular nodules, because of the more regular stratification of the rock in which they occur. Mentioning these, however, it may be added that overlying these very building stones and forming their 'bearing' is a mass of white, calcareous, soft rock with siliceous nodules, so exactly like the ordinary chalk and flint, that they might easily deceive a casual observer, and actually differing in scarcely anything but their fossils, and consequently their age.

But in this district there is one great quarry in which a higher stone is worked; this lies above the chalky rocks just mentioned, and thus is separated by them from the lower freestone. This higher stone is a very beautiful one; it has a brilliant white colour, and rings like a bell beneath a blow of the hammer; it is very free, very soft when first extracted, and large blocks may be obtained; moreover, to judge from the colour of the cast-out refuse, it does not easily discolour. It would seem, then, to have everything to recommend it; it is, however, somewhat coarse in the grain, and holes occur in it here and there; and, as far as can be seen, it must be very limited in quantity. Its high position in the series, and the succession of the overlying Purbecks, point it out to be on the same horizon as the true Portland Stone, so that the circumstances which favoured the production of a valuable quality may have here been repeated. The celebrated 'dirt bed,' with trees, of the Isle of Purbeck, is here repeated; the vegetable soil, the stems of the trees, but not as yet the roots.

The only remaining area in which Portland Stone is worked is the typical district which extends from Portland as far as Swanage on the east and Upway on the north. The upper surface of the Island of Portland may almost be described as one great quarry, so riddled is it everywhere by excava-

tions. The lower part of the 'stone,' resting at a considerable elevation on the 'sand,' is very flinty and of no use for building, though it represents, with the single exception above noted, the building stones of all the other districts. It is a remarkable circumstance that the flint is only developed at those places which have limestones of the same series overlying the beds, and the more of this overlying limestone there is the more flint is found. It is as if the flint were derived from this limestone and passed from it into the beds below. The upper part, however, of this flinty series contains beds which yield a very good stone, though not the best. Here the flints are few and far between, and only spoil the six or eight inches in which they lie. These beds are very inconstant, and, indeed, are more or less false-bedded; they increase in a few hundred yards to a workable thickness, and in a few hundred more become almost worthless.

The most valuable stone is known as the Whit or White bed; towards the east of the island this is seen to become false-bedded and to thin out, lying on a worn surface of the older series, but towards the west it attains a thickness of eight or ten feet, and is at its best. The grains of which this limestone is ordinarily composed are very small, and probably represent minute fragments of shells, though towards the top it is partially oolitic. Its qualities are too well known to need description, one of its chief peculiarities being its freedom from fossils; blocks of it give a fine bell-like tone, not, however, superior to that of the Tisbury stone. Over this in the island alone is found a remarkable rock, known locally as the 'roach.' It is wholly composed in one sense of shells, and yet in another sense there is not a shell in it. Originally it was a mass of shells, chiefly a doubtfully marine species, *Cerithium portlandicum*, or the 'Portland Screw.' These lay as close to one another as they could, the intervening spaces only being filled with some calcareous *débris*, subjected subsequently to some process similar to that which has dissolved the silex out of so many fossil sponge spicules and deposited it around them; the shells have all been dissolved, and only that part of the rock remains which was not shell, that is, the surrounding matrix and the internal cavity, so the roach is full of the holes where the shells have been, and contains models of their outside and inside. In this central spot—the last to emerge from out of the fostering sea—the enfeebled inhabitants of the period were congregated, and here they were together overwhelmed and found a common grave.

Although at so short a distance from Portland, neither Upway nor the Isle of Purbeck yield such good stone as the island. At the former place there is very little even to repre-

sent the building stone; almost all is chalky, and at the base flint-bearing. All along the coast from St. Alban's Head to Swanage one part or other of the series is found; but though the representatives of the upper stone may be recognized by a geologist, their value is obviously comparatively small. At the coast south of Worth-Maltravers, and at Tillym, stone is extracted, but it is obtained from a distinct bed, developed at a lower level than the White bed, and scarcely represented in the island. It is a very good freestone, white, and of fine grain, but large blocks have to be mined for.

These are the whole of the English localities where 'Portland Stone' is quarried, but the same rocks yield an abundant supply in the neighbourhood of Boulogne. Some of the stone here worked belongs undoubtedly to the same horizon as the lower beds at Portland, but the great quarries of Mont Lambert and Chatillon are said by the French geologists to belong to an earlier episode. Dr. Fitton was of a different opinion, and a hasty examination of them leads me rather to coincide with him in this. The similarity of the rocks to those at Swindon is remarkable in the extreme.

Such are the Portland building stones. One very remarkable feature about them is that they are not as a rule oolitic. Beds of very clean oolite do occur among them, consisting of the little rounded grains alone, with scarcely a particle of visible cement, but those that are of value have seldom any of these grains; they consist rather of exceedingly fine fragments, when any structure can be made out, derived from shells and other organisms. This peculiarity has been noticed by Mr. Sorby in his anniversary address to the Geological Society for 1879, and a remarkable addition made by a study of the microscopic structure. He states that the most abundant fragments are of Echinoderms, Brachiopoda, Ostreæ, Polyzoa, and Aviculæ. Now except the oyster itself, rarely associated with the actual building stone, all the others are exactly the classes of organisms which can seldom or never be found in the rocks. Echinoderms are of excessive rarity, while no one has ever seen to my knowledge a single Brachiopod, Polyzoan, or Avicula in the Portland limestone. The absence of Brachiopods especially is remarkable, though certainly they were not abundant in the Corallian limestones, yet in most limestones they are numerous. Did they exist and become universally ground to powder? or are the fragments of shells not those of animals living at the time, but derived from older rocks? If the Kimmeridge Clay were derived from the Lias, the Portland Stone might well be derived from the Bath Oolites. There is a singular absence, too, of corals; the accompanying absence of oolite lends strong support to the view that these two are intimately connected. These

observations on its structure lead to the conclusion that a large proportion at least of Portland Stone is derived not directly from broken-down shells forming calcareous mud, and subsequently consolidated, but from previously formed shelly limestones, either nearly contemporaneous or more remote in time. At all events, the name 'Portland Oolite' is to a great extent a misnomer, and for some of the rocks which are worked as building stones, 'Portland Limestone' is not much better. The great value of the stones is derived from their being so finely and uniformly ground, while their calcareous cement gives them the quality of many limestones of being easily worked, and these advantages are shared, though in a minor degree, by the beds at Worth, the Vale of Wardour, and Swindon.

CLIMBING PLANTS.*

By FRANCIS DARWIN, F.L.S.

I THINK most people have a general idea of what a climbing plant is. Even in the smoky air of London two representatives of the class flourish. A certain house in Portman Square shows how well the Virginian creeper will grow; and the ivy may be seen making a window-screen for some London dining-rooms.

Many other climbing plants will suggest themselves: the vine, the honeysuckle, the hop, the bryony, as forming more or less striking elements in the vegetation.

If we inquire what qualities are common to these otherwise different plants, we find that they all have weak and straggling stems, and that instead of being forced, like many weakly-built plants, to trail on the ground, they are all enabled to raise themselves high above it, by attaching themselves in some way to neighbouring objects. This may be effected in different ways; by clinging to a flat surface, like the ivy, or twining round a stick, like the hop, or making use of tendrils, like the vine.

These various contrivances have been studied by more than one German naturalist, as well as by my father, in whose book on the *Habits of Climbing Plants* very full details upon this subject will be found.

Climbing plants are, first of all, divided roughly into those which twine and those which do not twine; twiners are represented by the hop and the honeysuckle, and all those plants which climb up a stick by winding spirally round it. Those which are not twiners—that is, which do not wind spirally round a stick—are such as support themselves by seizing hold of any neighbouring object with various kinds of grasping organs; these may be simple hooks, or adhering roots, or they may be

* Founded on a Lecture delivered before the Sunday Lecture Society, Jan. 25th.

elaborate and sensitive tendrils, which seize hold of a stick with a rapidity more like the action of an animal than of a plant. We shall come back to this second-class of climbing plants, and shall then consider their various kinds of seizing organs. I merely wish now to insist on the importance of distinguishing between these two methods of climbing, in one of which the plant ascends a support by travelling spirally round it, in the other fixes on to the support by seizing it at one place, and continuing to seize it higher and higher up as its stem increases in length.

I have heard the curator of a foreign botanic garden bitterly complain of his gardeners that they never could learn the difference between these two classes of climbing plants, and that they would only give a few bare sticks to some tendril-bearing plant, expecting it to twine up them like a hop, while the plant really wanted a twiggy branch, up which it might creep, seizing a twig with each of its delicate tendrils, as it climbed higher and higher. These two kinds of climbers—twiners and non-twiners—may be seen growing up their appropriate supports in any kitchen-garden where the scarlet-runners twine spirally up tall sticks, while the peas clamber up the bushy branches stuck in rows in the ground.

A hop plant will supply a good example of the mode of growth of true twining plants. Let us imagine that we have a young hop plant growing in a pot; we will suppose that it has no stick to twine up, and that its pot stands in some open place where there are no other plants to interfere with it. A long thin shoot will grow out, and not being strong enough to support itself in the upright position, will bend over to one side. So far we have not discovered anything remarkable about our hop; it has sent out a straggling shoot, which has behaved, as might be expected, by falling over to one side. But now if we watch the hop plant closely, a very remarkable thing will be seen to take place. Supposing that we have noticed the shoot, when it began to bend over, pointed towards the window—say a north window—and that when we next look at it after some hours, it points into the room, that is to say, south, and again, north after another interval, we shall have discovered the curious fact that the hop-plant has a certain power of movement by which its shoot may sometimes point in one direction, sometimes in another. But this is only half the phenomenon, and if we examine closely, we shall find that the movement is *constant* and *regular*, the stem first pointing north, then east, then west, then south, in regular succession, so that its tip is constantly travelling round and round like the hand of a watch, making on an average, in warm August weather, one revolution in two hours. Here, then, is a most curious power possessed by the shoots of twining plants, which

is worth inquiring further into, both as regards the way in which the movement is produced, and as to how it can be of any service to the plant. Questions are often asked in gardening periodicals as to how hops or other climbing plants always manage to grow precisely in the direction in which they will find a support. This fact has surprised many observers, who have supposed that climbing plants have some occult sense by which they discover the whereabouts of the stick, up which they subsequently climb. But there is in reality no kind of mystery in the matter: the growing shoot simply goes swinging round till it meets with a stick, and then it climbs up it. Now a revolving shoot may be more than two feet long, so that it might be detained in its swinging-round movements by a stick fixed into the ground at a distance of nearly two feet. There would then be a straight bit of stem leading from the roots of the plant, in a straight line to the stick up which it twines, so that an observer who knew nothing of the swinging-round movement might be pardoned for supposing that the plant had in some way perceived the stick and grown straight at it. This same power of swinging round slowly comes into play in the very act of climbing up a stick.

Suppose I take a rope and swing it round my head: that may be taken to represent the revolving of the young hop-shoot. If, now, I allow it to strike against a rod, the end of the rope which projects beyond the rod curls freely round it in a spiral. And this may be taken as a rough representation of what a climbing plant does when it meets a stick placed in its way. That is to say, the part of the shoot which projects beyond the stick continues to curl inwards till it comes against the stick; and as growth goes on, the piece of stem which is projecting is, of course, all the while getting longer and longer; and as it is continually trying to keep up the swinging-round movement, it manages to curl round the stick. But there is a difference between the rope and the plant in this; that the rope curls round the stick at the same level as that at which it is swung, so that if it moves round in a horizontal plane at a uniform height above ground, it will curl round the stick at that level, and thus will not climb *up* the stick it strikes against. But the climbing plant, although it may swing round when searching for a stick, at a fairly uniform level, yet when it curls round a stick, does not retain a uniform distance from the ground, but by winding round like a corkscrew it gets higher and higher at each turn.

One may find a further illustration of the action of twining in the swinging-rope model. It is a peculiarity of twining plants that they can only ascend moderately thin supports. A scarlet-runner can climb up a bit of string, or a thin stick, an inch or

two in diameter, but when it comes to anything thicker than this, it fails to do so. Just as when the swinging-rope strikes against a large trunk of a tree, it would be unable to take a turn round it, and would fall to the ground instead of gripping it with a single turn, as it does a thin stick. The difficulty which a climbing plant has in ascending a thick stick will be better understood by going back to the original swinging round movement which the plant makes in search of a stick, and considering how the movement is produced.

As plants have no muscles, all their movements are produced by unequal growth; that is, by one-half of an organ growing in length quicker than the opposite half. Now the difference between the growth of a twining plant which bends over to one side, and an ordinary plant which grows straight up in the air, lies in this, that in the upright shoot the growth is nearly equal on all sides at once, whereas the twining plant is always growing much quicker on one side than the other.

It may be shown by means of a simple model, how unequal growth can be converted into revolving movement. The stem of a young hop is represented by a flexible rod, of which the lower end is fixed, the upper one being free to move. At first the rod is supposed to be growing vertically upward, but when it begins to twine, one side begins to grow quicker than any of the others; suppose the right side to do so, the result will be that the rod will bend over towards the left side. Now let the region of quickest growth change, and let the left side begin to grow quicker than all the others, then the rod will be forced to bend back over to the other side. Thus, by an alteration of growth, the rod will bend backwards and forwards from right to left. But now imagine that the growth of the rod on the sides nearest to and furthest from us enter into the combination, and that after the right side has been growing quickest for a time, the far side takes it up, then the rod will not bend straight back towards the right, as it did before, but will bend to the near side. Now the old movement caused by the left side growing quickest, will come in again, to be followed by the near side growing quickest. Thus by a regular succession of growth on all the sides, one after another, the swinging-round movement is produced, and by a continuation of this action, as I have explained, the twining movement is produced.

I have spoken as if the question of how plants twine were a completely solved problem, and in a certain sense it is so. I think that the explanation which I have given will remain as the fundamental statement of the case. But there is still much to be made out. We do not in the least know why every single hop-plant in a field twines like a left-handed screw,

while every single plant in a row of beans twines the other way; nor why in some rare instances a species is divided, like the human race, into right and left-handed individuals, some twining like a left-handed, others like a right-handed screw. Or, again, why some very few plants will twine half-way up a stick in one direction, and then reverse the spiral and wind the other way. Nor though we know that in all these plants the twining is caused by the change in the region of quickest growth, have we any idea what causes this change of growth. There is still much to work at, and it is to be hoped that there are still plenty of workers to solve the problems. It is by looking to exceptions that the key to a problem is often found. It is the exceptions to general rules that often lead us to understand the meaning and origin of the rules themselves; and it is to such exceptions that any one who wants to work at climbing plants should turn. Now, it is a general rule that a climbing plant twines in the same way that it revolves. It seems an obvious thing that in the case of the rope model, if we swing the rope round our head in the direction of the hands of a watch, it must twine round the stick against which it strikes in the same direction. But in plants it is not always so. In the large majority of cases it is so, for if this were not the case, the illustration of the rope would not have been applicable; but it is not universally the rule. Every individual of the plant Hibbertia always twines round its stick in the same direction, but when it is performing the swinging movement in search of a support, it is found that some plants travel round with the sun, others in the opposite direction. This fact forms an exception of a striking kind—and such exceptions are worthy of close study.

There are other facts of a different nature, which seem to show how difficult the problem is, and how delicately balanced is that part of the organization of the plant which is connected with the power of climbing. For instance, if we cut a branch of most shrubs, and put it in water, it goes on growing, apparently as healthily as ever. Indeed the practice of making cuttings—where a cut-off branch or shoot developes roots and turns into a new plant—shows us that no serious injury is thus caused. But the twining organization is sensitive to such treatment. A cut branch of hop placed in water was observed to make its revolutions in about twenty hours, whereas in its natural condition—growing on the plant—it makes a complete turn in two or three hours. Again, if a plant growing in a pot is moved from one green-house to another, the slight shaking thus caused is sufficient to stop the revolving movement for a time,—another proof of the delicacy of the internal machinery of the plant.

Some of the problems, as, for instance, why twining plants cannot as a rule climb thick stems, may be looked at from the natural-history point of view. Most of our climbing plants die down in the winter, so that if they were able to climb round big tree trunks, they would waste all the precious summer weather in climbing a few feet, whereas the same amount of longitudinal growth devoted to twining up a thin stick would have raised them up to the light after which they are striving. And as a plant exercises no choice, but merely swings round till it hits against an object, up which it will then try to twine, it seems as if the inability to climb thick stems might be a positive advantage to a plant, by forcing it to twine up such objects as would best repay the trouble.

In the classification of climbing plants, proposed by my father in his book, he makes a subdivision of 'hook-climbers.' These may be taken as the simplest representatives of that class of climbers which are not twining plants. The common bramble climbs or scrambles up through thick underwood, being assisted by the re-curved spines which allow the rapidly-growing shoot to creep upwards as it lengthens, but prevent it from slipping backwards again; the common goose-grass, (Galium) also climbs in this way, sticking like a burr to the side of a hedge-row up which it climbs. Most country boys will remember having taken advantage of this burr-like quality of Galium in making sham birds' nests, the prickly stems adhering together in the desired form. Such plants as the bramble or Galium exhibit none* of the swinging round movement which I have described in twiners: they simply grow straight on, trusting to their hooks to retain the position gained.

In some species of Clematis we find a mechanism, which reminds one of a simple hook climber, but is in reality a much better arrangement. The young leaves projecting outwards and slightly backwards from the stem, may remind us of the hooked spines of a bramble, and like them easily catch on neighbouring objects, and support the trailing stem. Or the leaf of the species of Clematis given in Fig. 1, may serve as an example of a leaf acting like a hook. The main stalk of the leaf is seen to be bent angularly downwards at the points where each successive pair of leaflets is attached, and the leaflet at the end of the leaf is bent down at right angles, and thus forms a grappling apparatus. The Clematis does not, like the bramble, trust to mere growth, to thrust itself among tangled bushes, but possesses the same powers of revolving in search of

* That is to say, the revolving movement is not sufficiently developed to be of practical importance. The same remark is applicable to the other cases in which I have spoken of the absence of revolving movement in the growing parts of plants.

a support which, simple or true twining plants possess. Indeed, many species of Clematis are actually twining plants, and can wind spirally up a stick placed in their way. And the same revolving movement which enables them thus to wind spirally, also helps them to search for some holding place for their hook- or grapple-like leaves, and in many species the search is carried on by the leaves swinging round, quite independently of the revolving movement of the stem on which they are borne.

If a leaf of a Clematis succeed by any means in hooking on to a neighbouring object, the special characteristic of leaf-climbing plants comes into play. The stalk of the leaf curls strongly over towards the object touching it, and clasps it firmly. It is obvious how great is the advantage thus gained over a mere hook. A leaf such as that shown in Fig. 2 might

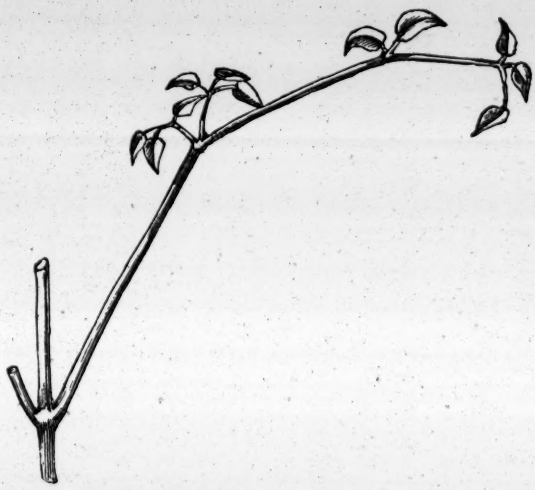


FIG. 1.*
A young leaf of *Clematis viticella*.

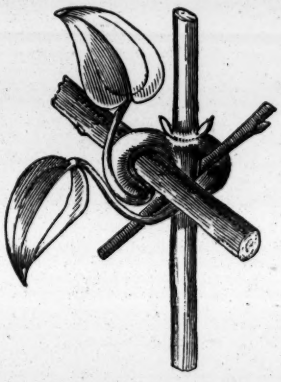


FIG. 2. *Clematis glandulosa*.
With two young leaves clasping two twigs, with the clasping portions thickened.

be made to catch on to a neighbouring twig by its bent stalk, in such a way, that although it managed to stay where it was, it could bear none of the weight of the plant, and would be liable to be displaced by a strong wind or other disturbance. But when the stalk of the leaf had curled close round the twig, nothing could displace it, and it could take its share in the work of supporting the plant.

The extreme sensitiveness of the leaf-stalk to slight and gentle touches, gives a curious idea of the alertness of the plant in its search for supporting objects. A leaf may be excited to bend, by a loop of string weighing only $\frac{1}{16}$ grain. It is an

* For the loan of this and the other woodcuts illustrating this article, we are indebted to the kindness of Mr. Charles Darwin and Mr. Murray.

interesting fact that, in such a hook-like leaf as that of *Clematis viticella* (Fig. 1), the hooked end of the leaf, which has the best chance of coming into contact with obstacles, is the most sensitive part. This has been made out by hanging small weights on different parts of the leaf, and it is found that the terminal leaflet bends in a few hours after a loop of string weighing less than a grain is hung on it, and which produced no effect in twenty-four hours on the other petioles. One may see proof of the sensitiveness of the leaf-stalks of the wild English Clematis, which sometimes catches withered leaves or delicate stalks of the quaking grass. The same thing is shown by a leaf after having been touched with a little water-colour, the delicate crust of dry paint being mistaken for something touching the plant. In such cases, or when the leaf has been merely rubbed with a twig, which is taken away before the leaf seizes it, the plant discovers that it has been deceived, and after bending for a time, it unbends and becomes straight again.

The bending, which enables a leaf to seize a twig, is not the only change which the stimulus of a touch produces. The leaf-stalk swells and becomes thicker and more woody, and turns into a strong, permanent support to the plant. The thickening of the leaf-stalks is to be made out in Fig. 2, which represents a shoot of clematis, bearing two leaves, each of which has seized a twig; in one of the leaf-stalks this thickening has commenced, and is fairly evident. The thickened and woody leaf-stalks remain in winter after the leafy part has dropped off, and in this condition they are strikingly like real tendrils.

The genus Tropæolum, whose cultivated species are often called Nasturtiums, also consists of leaf-climbing plants, which climb like Clematis by grasping neighbouring objects with their leaf-stalks.

In some species of Tropæolum we find climbing organs developed, which cannot logically be distinguished from tendrils; they consist of little filaments, not green like a leaf, but coloured like the stem. Their tips are a little flattened and furrowed but never develope into leaves; and these filaments are sensitive to a touch, and bend towards a touching object, which they clasp securely. Filaments of this kind are borne by the young plant, but it subsequently produces filaments with slightly enlarged ends, then with rudimentary or dwarfed leaves, and finally with full-sized leaves; when these are developed they clasp with their leaf-stalks, and then the first-formed filaments wither and die off; thus the plant, which in its youth was a tendril-climber, gradually developes into a true leaf-climber. During the transition, every gradation between a leaf and a tendril may be seen on the same plant.

It is not always the stalk of a leaf which is developed into the clasping organ; the Bignonia leaf shown in Fig. 3 bears tendrils at its free extremity. And in other plants tendrils are formed from flower-stalks, in which the flowers are not developed, or the whole stem of the plant or a single branch may turn into a tendril. In one curious case of monstrosity, what should have been a prickle on a sort of cucumber, grew out into a long, curled tendril.

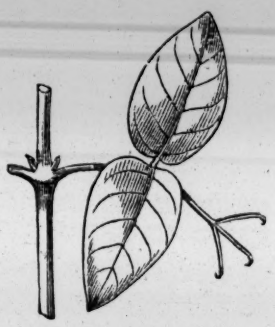


FIG. 3. *Bignonia.* An unnamed species from Kew.

The family of the Bignonias is one of the most interesting of the class of tendril-climbers, on account of the variety of adaptation which is found among them.

In the above-mentioned Fig. 3 is seen the tendril-bearing leaf of a species of Bignonia. The leaf bears a pair of leaflets, and ends in a tendril having three branches. The main tendril may be compared to a bird's leg with three toes, each bearing a small claw. And this comparison seems apt enough, for when the tendril comes against a twig, the three toes curl round it like those of a perching bird.

Besides the toes or tendrils, the leaf-stalk is sensitive, and acts like that of a regular leaf-climber, wrapping itself round a neighbouring object.

In some cases the young leaves have no tendrils at their tips, but clasp with their stalks, and this is a case exactly the reverse of Tropæolum—a tendril-climber whose young leaves have no tendrils, instead of a leaf-climber whose young climbing organs are not leaves. Thus the close relationship that exists between leaf and tendril-climbers is again illustrated.

This plant also combines the qualities of another class of climbers, namely twiners, for it can wind spirally round a support as well as a hop or any other true twiner. Another species, *B. Tweedyana*, also helps to support itself by putting out roots from its stems, which adhere to the stick up which the plant is climbing. So that here are four different methods of climbing, twining, leaf, tendril, and root climbing, which are usually characteristic of different classes of climbing plants, combined in a single species.

Among the Bignonias are found tendrils with various curious kinds of sensitiveness. The tendrils of one species exhibit, in the highest perfection, the power of growing away from light towards darkness, just the opposite to the habit of most plants. A plant, growing in a pot, was placed so that the light came in on one

side. One tendril was pointing away from the light to begin with, and this did not move; but the opposite tendril, which was pointing towards the light, bent right over, and became parallel to the first tendril. The pot was then turned round, so that both pointed towards the light, and they both moved over to the other side, and pointed away from the light. In another case, in which a plant, with six tendrils, was placed in a box, open at one side, all six tendrils pointed like so many weather-cocks in the wind—all truly towards the darkest corner of the box. These tendrils also showed a curious power of choice. When it was found that they preferred darkness to light, it was tried whether they would seize a blackened glass-tube, or a blackened zinc plate. The tendrils curled round both these objects, but soon recoiled, and unwound with, what my father says, he can only describe as disgust. A post with very rugged bark was then put near them; twice they touched it for an hour or two, and twice they withdrew; but at last one of the hooked tendrils caught hold of a little projecting point of bark; and now it had found what it wanted. The other branches of the tendril quickly followed it, spreading out, adapting themselves to all the inequalities of the surface, and creeping into all the little crevices and holes in the bark. Finally a remarkable change took place in the tendrils: the tips which had crept into the cracks, swelled up into little knobs, and ultimately secreted a sticky cement, by which they were firmly glued into their places. This plan of forming adhesive discs on its tendrils is one which we shall find used by the Virginian Creeper, as its only method of support, and it forms the fifth means of climbing to be met with among the Bignonias. We see now the meaning of the power possessed by the tendrils of moving towards the dark, for in this way they are enabled to find out and reach the trunks of trees to which they then become attached. It seems, moreover, that the tendrils are especially adapted to the moss or lichen-covered trees, for the tendrils are much excited by wool, flax, or moss, the fibres of which they can seize in little bundles. The swelling process is so delicate, that when two or three fine fibres rest on the end of a tendril, the swelling occurs in crests, thinner than a hair, which insert themselves between, and finally envelope the fibres. This goes on so that the ball at the end of a tendril may have as many as fifty or sixty fibres imbedded in it, crossing each other in different directions.

The tendrils of the Virginian Creeper may here be worth noticing. This plant can climb up a flat wall, and is not adapted to seize sticks or twigs; its tendrils do occasionally curl round a stick, but they often let go again. They, like the Bignonia tendrils, are sensitive to the light, and grow away

from it, and thus easily find out where the wall lies, up which they have to climb. A tendril which has come against the wall is often seen to rise and come down afresh, as if not satisfied with its first position. In a few days after a tendril has touched a wall the tip swells up, becomes red, and forms one of the little feet or sticky cushions by which the tendrils adhere, and which are shown in Fig. 5. The adherence is caused by a resinous cement secreted by the cushions, and which forms a strong bond of union between the wall and the tendril. After the tendril has become attached it becomes woody, and is in this state remarkably durable, and may remain firmly attached and quite strong, for as many as fifteen years.

Besides this sense of touch, by which a Bignonia tendril distinguishes between the objects which it touches, there are other instances of much more perfect and incomprehensible sensibility. Thus some tendrils, which are so sensitive that they curl up when a weight of $\frac{1}{30}$th or even $\frac{1}{60}$th grain is placed on them, do not take the least notice of a shower of rain whose falling drops must cause a much greater shock to the tendrils.

Again, some tendrils seem to have the power of distinguishing between objects which they wish to seize, and their brother tendrils which they do not wish to catch. A tendril may be drawn repeatedly over another without causing the latter to contract.

The tendrils of another excellent climber, *Cobæa scandens*, possess some curious properties. The tendrils are much divided, and end in delicate branchlets, as thin as bristles, and very flexible, each bearing a minute double hook at its tip. These are formed of a hard, woody substance, and are as sharp as needles; a single tendril may bear between ninety and a hundred of these beautiful little grappling-hooks. The flexibility of the tendrils is of service in allowing them to be blown about by a breath of wind, and they can thus be made to seize hold of objects which are out of reach of the ordinary revolving movements. Many tendrils can only seize a stick by curling round it, and this even in the most sensitive tendril must take a minute or two; but with Cobæa, the sharp hooks catch hold of little irregularities on the bark the moment the tendril comes into contact with it, and afterwards the tendril can curl round and make the attachment permanent. The importance of this power of temporary attachment is shown by placing a glass rod near a Cobæa plant. Under these conditions the tendrils always fail to get hold of the glass, on which its grapple-like hooks cannot seize.

The movement of the little hook-bearing branches is very remarkable in this species. If a tendril catches an object with

one or two hooks, it is not contented, but tries to attach the rest of them in the same way. Now many of the branches will chance be so placed that their hooks do not naturally catch, either because they come laterally, or with their blunt backs against the wood, but after a short time, by a process of twisting and adjusting, each little hook becomes turned, so that its sharp point can get a hold on the wood.

The sharp hook on the tendrils of Cobæa is only a very perfect form of the bluntly curved tip which many tendrils possess, and which serves the same purpose of temporarily holding the object caught until the tendril can curl over and make it secure. There is a curious proof of the usefulness of even this blunt hook in the fact that the tendril is only sensitive to a touch on the inside of the hook. The tendril, when it comes against a twig, always slips up it till the hook catches on it, so that it would be of no use to be sensitive on the convex side. Some tendrils, on the other hand, have no

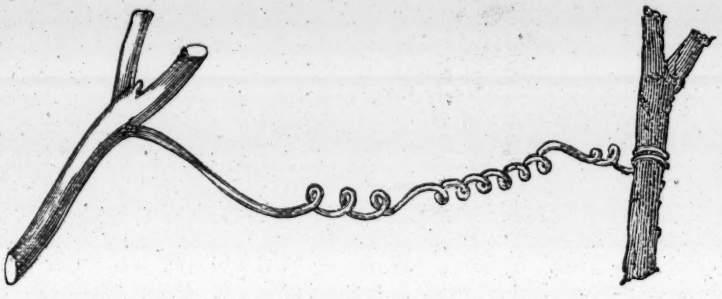


FIG. 4.

A caught tendril of *Bryonia dioica*, spirally contracted in reversed directions.

hook at the end, and here the tendrils are sensitive to a touch on any side. These tendrils led my father at first into a curious mistake, which he mentions in his book. He pinched a tendril gently in his fingers, and finding that it did not move, concluded it was not sensitive. But the fact was that the tendril being touched on two sides at once, did not know which stimulus to obey, and therefore remained motionless. It was in reality extremely sensitive to a touch on any one of its sides.

There is a remarkable movement which occurs in tendrils after they have caught an object, and which renders a tendril a better climbing organ than any sensitive leaf. This movement is called the 'spiral contraction,' and is shown in Fig. 4, which represents the spirally contracted tendril of the wild Bryony; it may also be seen in Fig. 5, which represents the tendril of the Virginian Creeper. When a tendril first seizes an object it is quite straight, with the exception of the extreme tip, which is firmly curled round the object seized. But in a

day or two the tendril begins to contract, and ultimately assumes the cork-screw-like form represented in the figures. It is clear that in spirally contracting the tendril has become considerably shorter; and since the end of the tendril is fixed to a branch, it is obvious that the stem of the bryony must be dragged nearer to the object which its tendril has caught.

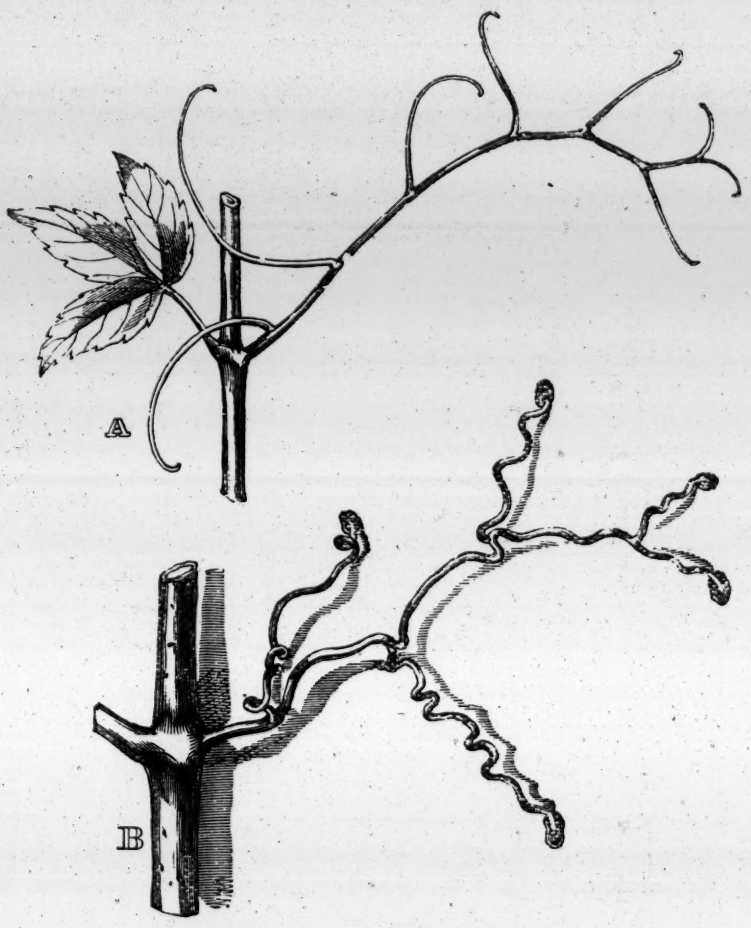


FIG. 5. *Ampelopsis hederaria.*

A. Tendril fully developed, with a young leaf on the opposite side of the stem.

B. Older tendril, several weeks after its attachment to a wall, with the branches thickened and spirally contracted, and with the extremities developed into discs. The unattached branches of this tendril have withered and dropped off.

Thus, if a shoot of bryony seizes a support above it, the contraction of the tendril will pull up the shoot in the right direction. So that in this respect the power of spiral contraction gives a tendril-climber an advantage over leaf-climbers which have no contracting power, and therefore no means of hauling themselves up to supporting objects.

But the spiral contraction of tendrils has another use,

and this is probably the most important one. This use depends on the fact that a contracted tendril acts like a spiral spring, and is thus converted into a yielding, instead of an unyielding, body. The spirally-wound tendril yields like an elastic thread to a pull which would break the tendril in its original condition. The meaning of this arrangement is to enable the plant to weather a gale which would tear it from its support by snapping the tendrils, if they were not converted into spiral springs.

My father describes how he went in a gale of wind to watch the bryony on an exposed hedge, and how, in spite of the violent wind which tossed the branches of the plant about, the bryony safely rode out the gale, 'like a ship with two anchors down, and with a long range of cable a-head, to serve as a spring as she surges to the storm.' It may also serve to divide the weight which has to be supported equally among a number of tendrils; and this is the meaning of the spiral contraction seen in the tendrils of the Virginia Creeper.

It can be seen in Fig. 4 that all the coils of the spiral are not in the same direction. First, there are two in one direction, then six in the other, and then three again in the first direction, making six turns in one way and five in the other. And this is universally the case; the turns in one direction are always approximately equal in number to those in the opposite direction. It can be shown to be a mechanical necessity that a tendril which has its two ends fixed, and which then coils into a spiral, should behave in this way.

A simple model made to show this mechanical necessity is described by Sachs in his Text Book. It is made by stretching a strip of indiarubber and cementing it to an unstretched strip. The strips being united in a state of longitudinal strain, form a spiral when released. If the model is held by one end only, the turns of the spiral are all in one direction. And this represents the behaviour of a tendril which has not managed to seize a support; for some unknown reason such tendrils contract into spirals, and the turns of such spirals are all in one direction. But if the india-rubber is held at both ends, half the turns are in one direction, half in the other, just as with a tendril the same thing happens.

Now let us consider the general relations that exist between twining plants, leaf-climbing plants, and tendril-climbing plants. To an evolutionist the question how these various classes of climbing plants have been developed is perhaps of most interest. What is the relationship between them? Have all classes been developed separately from ordinary non-climbing plants, or has one class been developed out of one of the others; and if so, which is the oldest form of climbing

plant? There can be little doubt on this latter point. I think we may certainly say that the earliest form which existed was a twining plant. We see that twining plants do not possess the essential feature of leaf or tendril-bearers, namely, the sensitiveness to a touch, which enables a leaf or tendril to grasp a stick. But, on the other hand, most leaf and tendril climbers do possess the essential quality of a twiner—the power of revolving or swinging round, which exists in the shoots, leaves, or tendrils of so many of them. This power of revolving merely serves in some leaf and tendril climbers to carry on the search for supports; but other leaf and tendril climbers, as we have seen, do actually wind spirally round a stick exactly like a true twiner. How twiners originally obtained their power of swinging round we need not now inquire; it seems to be merely an increase of a similar movement which is found to occur in a meaningless manner in other plants. Thus several flower-stems have been observed bowing themselves over and swinging round in small circles, like climbing plants. Here the movement is merely an unintelligible concomitant of growth, for, as we see, the movement is of no advantage to the flower-stem. But the existence of this movement is of great interest to us, for it shows how a twining plant might be developed by a similar movement being found to be advantageous, and being increased by natural selection to the requisite extent.

Another question which may occur to us is this: in what way is climbing by leaves or tendrils a more perfect method than twining? Why, when a plant had become a twining plant, did it not rest satisfied? The fact that leaf and tendril-climbers have been developed out of twiners, and not *vice versâ*, is a proof that climbing by leaves or tendrils is a more advantageous habit than twining; but we do not see why it should be so. If we inquire why *any* plant has become a climber, we shall see the reason. Light is a necessity for all green plants; and a plant which can climb is enabled to escape from the shadow of other plants with a far less waste of material than a forest-tree, which only pushes its branches into the light by sheer growth. Thus the weak, straggling stem of a climbing plant gets all the advantages gained by the solid, column-like tree trunk. If we apply this test,—which is the most economical plan of climbing, twining or leaf-climbing,—we see at once that a plant which climbs by seizing wastes far less material than one which twines. Thus a kidney-bean which had climbed up a stick to a height of two feet, when unwound from its support was found to be three feet in length, whereas a pea which had climbed up two feet by its tendrils was hardly longer than the height reached. Thus the bean had wasted

considerably more material by its method of climbing by twining round a stick, instead of going straight up, supported by its tendrils, like the pea. There are several other ways, in which climbing by tendrils is a much better plan than twining. It is a safer method, as any one may convince himself by comparing the security of a tendril-bearer in a heavy wind, with the ease with which a twiner is partly blown from its support. Again, by looking at those leaf-climbing plants which still possess in addition the power of twining, it will be seen how incomparably better they grasp a stick than does a simple twiner. And again, a twiner from being best fitted to climb bare stems often has to start in the shade, whereas a leaf or tendril climber can ramble for the whole extent of its growth up the sunny side of a bush.

We can thus see plainly how it has been an advantage for twining plants to develope into leaf-climbers. We shall also find reasons why a leaf-climber should find it advantageous to become a tendril-climber.

We have seen how tendrils form a more sensitive, efficient grasping organ, than simple leaves. Tendrils possess also the valuable power of shortening themselves by spirally contracting, and thus pulling up after them the stem on which they grow; and afterwards serving as springs and breaking the force of the wind. We have had some cases where we see the close relationship between leaf and tendril-climbers, and where we can see intermediate stages in the process of transition from one method of climbing to the other.

In certain kinds of Fumaria we can follow the whole process. Thus we have one kind, which is a pure leaf-climber, grasping by its leaf-stalks, which bear leaflets not at all reduced in size. A second genus has the end leaflets very much smaller than the rest. A third kind has the leaflets reduced to microscopical dimensions; and lastly, a fourth kind has true and perfect tendrils. If we could see the ancestors of this last kind we should undoubtedly have a series of forms connecting it with an extinct leaf-climber, resembling the series which at present connects it with its contemporary leaf-climbing relatives.

To repeat once more the steps which it is believed have occurred in the evolution of climbing plants. It is probable that plants have become twiners by exaggerating a swinging-round or revolving movement, which occurred in a rudimentary form, and in a useless condition, in some of their ancestors. This movement has been utilized for twining, the stimulus which has driven the process of change in this direction having been the necessity for light.

The second stage has been the development of sensitive

leaves by a twining plant. No doubt at first no leaf-climber depended entirely on its leaves, it was merely a twiner which helped itself by its leaves. Gradually the leaves became more perfect, and then the plant could leave off the wasteful plan of growing spirally up a stick, and adopt the more economical and more effective one of pure leaf-climbing.

Finally, from sensitive leaves were developed the marvellously perfect tendrils which can perceive $\frac{1}{50}$th of a grain, and can show distinct curvature within 25″ after being touched, tendrils, with delicate sticky ends, or endowed with the power of moving towards the dark, or of creeping into little cracks, or with that mysterious sense of touch by which a tendril can distinguish a brother tendril from an ordinary twig, and can distinguish the weight of a drop of rain hanging to it from a bit of thread—in short, all the delicate contrivances which place tendril-bearers so eminently at the head of the climbing plants.

There is only one more fact connected with the evolution of climbing plants which must be alluded to, namely, the curious way in which the representatives of the class are scattered throughout the vegetable kingdom. Lindley divided flowering plants into fifty-nine classes, called Alliances, and in no less than thirty-five of these climbing plants are found. This fact shows two things: first, how strong has been the motive power—the search after light—which has driven so many distinct kind of plants to become climbers. Secondly, that the power of revolving, which is the first step in the ladder of development of the power of climbing, is present in an undeveloped state in almost every plant in the vegetable series.

ON THE INFLUENCE WHICH A MOLECULAR MOVEMENT DUE TO ELECTRICITY MAY HAVE EXERTED IN CERTAIN GEOLOGICAL PHENOMENA, NAMELY, THE METAMORPHISM OF ROCKS AND THE FORMATION OF METALLIFEROUS DEPOSITS.*

By MANUEL FERNANDEZ DE CASTRO.

GEOLOGISTS of the present day admit three distinct theories concerning the formation of metalliferous deposits: 1. One theory for metalliferous deposits, such as pockets, veins, stockwerks, and segregated lodes, the grouping or concentration of which is ascribed to molecular action, but without explaining the origin of the latter. 2. Another theory for true plutonic deposits, including also almost all lodes of contact and often showing molecular grouping. 3. A third theory for the true or concretionary lodes, the component parts of which are believed by some geologists to have been brought, dissolved by thermal waters, from the interior of the earth. Some geologists, however, consider these lodes due to segregation.

The question arises: Is there no possibility of establishing a closer relation between these three classes of deposits?

Although many of the phenomena observed in mineral deposits could be more satisfactorily explained by the hydrothermal than by the plutonic theory, the former left many difficulties. Amongst others, as pointed out by De la Beche, the difference noted in the composition or structure of the lodes according to the nature and character of the rock traversed; and further, the fact that we may find in the mass of the deposit, close to substances due to the agency of water, others which could be produced artificially only in the dry way, and at very high temperatures. The last-mentioned difficulty has been overcome to a certain extent by Senarmont, who succeeded in producing, by the agency of water, at a temperature varying

* Extract from a pamphlet, *Discursos leidos ante la Real Academia de Ciencias, etc. en la recepcion publica del Exmo. Sr. Don Manuel Fernandez de Castro.* Madrid, 1878.

from 130° to 200° C., the principal characteristic minerals of lodes, such as quartz, spathic iron ore, heavy spar, and ruby silver. By far the greatest support, however, was given to the hydrothermal theory by the remarkable discovery, in the walls of a Roman aqueduct at the thermal springs of Plombières, of a Roman bronze key, covered with crystals of copper-glance, identical with those occurring in the mines of Cornwall. The waters of Plombières have a temperature of 70° C., and contain small quantities of silicates and sulphates of potash and soda. Crystals of other minerals, such as hyalite, apophyllite, and chabasite, were discovered in cavities of the mortar, but are not found in the porphyritic granite, from which these thermal waters flow. Daubrée carefully studied the occurrence of these minerals at Plombières, and, by experiments made in consequence, he proved that by means of water under a high pressure and at a temperature of 400° C., not only can quartz be obtained in crystals, as already shown by Senarmont, but also crystals of anhydrous silicates, such as felspar, diopside, and wollastonite. Daubrée considers the formation of the greater portion of the metalliferous lodes as a particular case of metamorphism, chiefly due to the concurrent action of heat, water, and pressure. These conclusions were put forth in an essay read before the Academy of Sciences of Paris in 1859; but ten years later the same scientific body awarded a prize to Delesse, who had arrived at the conclusion, that the causes of general or local metamorphism are those met when penetrating into the interior of the globe, viz., heat, water, pressure, and, above all, molecular action.

The formation of the minerals found at Plombières has taken place, however, without a pressure above that of the atmosphere, and without the direct action of the central heat of the globe, and is undoubtedly due to the slow but continuous action of a very small quantity of water of a temperature not above 70° C. The only manner in which the formation of these various minerals can be explained is by molecular action, as first suggested by Elie de Beaumont. As regards the nature and origin of this so-called molecular action, no explanation is given by the various authors; and De Castro finds this rather remarkable, considering that one author cites in his writings Becquerel, who as early as 1823 had demonstrated the influence of slow actions produced by electricity of a very feeble tension in connection with the precipitation of insoluble combinations similar to those found in nature. This manner of considering electrical reactions between molecule and molecule, quite different from that generally current, is worthy of attention, because it may be said that all the theoretical reasoning founded upon the known effects of electricity has been justified

by experience. These effects are as manifold as the causes which produce electricity, or as the circumstances under which it is set free and propagated. Considering the results obtained by means of electro-chemical action from the time that Nicholson and Carlisle decomposed water, and Davy extracted potassium and sodium by means of the voltaic pile, up to the time when Despretz obtained real crystals of diamond with the Ruhmkorff coil; and considering further the numerous crystallized substances, identical with natural ones, artificially produced by Becquerel and Crosse; it would be unreasonable to reject the idea of establishing a theory of metamorphism based upon electrochemical and electrodynamical actions. All the effects due to metamorphism can also be produced by electro-telluric action. Thus Alcide d'Orbigny, when treating of fossilization in his well-known *Cours Elémentaire de Paléontologie*, says that electrical actions and chemical affinities furnish excellent means for explaining substitution in fossils; and he adds at the same time, that the action of these occult forces is more general than has hitherto been believed. Becquerel succeeded in reproducing by electrochemical processes epigenies of petrefactions, which cannot be produced artificially in any other way; and when enumerating the causes to which metalliferous lodes are due, Fox and Fournet speak of electrochemical actions resulting from the contact of a number of various rocks.

The author's attention always has been drawn to certain geological phenomena which can be reproduced artificially in the laboratory of the chemist, but only by employing much more powerful means than nature apparently makes use of for the same purpose. Although we are able to perceive in both cases the effects, it is different as regards the *modus operandi*, because our senses are only capable of distinguishing and appreciating phenomena within a limited scale; and as soon as we have to consider something very great or very small, very quick or very slow, we require the use of instruments which increase the faculty of perception: but in the greater number of cases we can thus realize, by our imagination only, the nevertheless well-established facts. As already mentioned in the artificial production of the minerals found at Plombières, a much higher temperature and pressure had to be used than nature apparently employed in their formation. Thus, Wöhler had to make use of a temperature of 180°–190° C., and of a pressure of from ten to twelve atmospheres to produce artificially those minerals which nature had formed in the Roman aqueducts of Plombières at a temperature of 70° C., and under ordinary atmospherical pressure. Is it possible that nature, operating by infinitesimally small actions, but during an unlimited time, can achieve results astonishing by their magnitude, just as in the higher

mathematics, by the integration of the infinitesimally small, real and absolute quantities are obtained? Tyndall, in his examination of the origin and probable destiny of the Niagara falls, when speaking of the wear and tear due to running water, states that time and intensity are the principal factors of geological change, and, to a certain extent, convertible into each other. A weak force acting during a long period, and a more intense one acting during a shorter period, may approximately produce the same effect. The sand-blast, invented by Quincey, is an example of the concentration of force in space; and the reciprocal action of steel and flint, an illustration of the same principle. Although the total amount of heat produced by striking softer substances with each other, such as calc-spar and lead, may be even greater, the generation of a spark requires a local concentration of heat. This local concentration of heat is also obtained in electro-calorific actions. By the use of a Voltaic pile of 2000 pairs, Sir H. Davy succeeded in melting platinum; and though it can be demonstrated that, in a given time, the chemical action taking place in this pile is far from being equivalent to the fuel consumed in the same time in a blast-furnace, the heat generated in the latter is insufficient to melt the platinum-wire, which melts after being exposed for some seconds between the electrodes of the Voltaic pile. The Voltaic arc acts similarly to a powerful blowpipe, which heats and melts in rapid succession the particles of platinum; whereas, in a blast-furnace, the heat generated cannot accumulate in a given point, and therefore is incapable of melting the platinum, *i.e.*, of producing, in each of its molecules, the number of vibrations which are necessary for its melting.

If the possibility of calorific effects of the electric currents is admitted in the same manner as the electrochemical effects are already universally accepted for the purpose of explaining the slow reactions which operate in the earth's interior, all the principal difficulties presented by local and contact metamorphism disappear, and it then becomes possible to explain, without having need of any violent theory, such notable cases as, for example, that of the Blaue Kuppe, near Eschwege, to which Delesse repeatedly draws attention in his *Etudes sur la Métamorphisme*. In that locality, well known to geologists, is found a basaltic conglomerate which encloses a great number of small fragments of a variegated sandstone, which are all vitrified on their outside. This effect cannot be attributed to the basalt, because the heat retained by such a comparatively small mass would not have been sufficient to melt the exterior of the sandstone fragments. Delesse, therefore, arrives at the conclusion that an agent more subtle than the basalt has penetrated the sandstone, and produced the same liquidity by fusion.

According to his idea, this agent was water; but De Castro considers it not more venturous to attribute the phenomena of the Blaue Kuppe to the calorific action of the electro-chemical and electrodynamical currents, produced by that same water infiltrating into the sandstone, or by the different temperature of the contact rocks, or by any other of the many causes which incessantly originate analogous currents. For the explanation of local metamorphism, extending over large districts situated at a great distance from the fluid interior of the earth or the eruptive rocks, and sometimes containing interstratified, not altered rocks, the hydrothermal theory cannot be admitted without exaggerating the physical properties of the substances, and attributing to them properties which we are not aware of. But in the case of local metamorphism, all effects due to molecular action are observed—effects, which are all capable of producing electro-telluric currents. An electrical current, by whatever cause produced, be it by the contact of two rocks of a different temperature, or the slow circulation of water more or less charged with other substances, or the very act itself of rending or fissuring the earth's crust by the contraction or sliding of the rocks, or the presence of some organic substance in the layers of the formation, or many other causes, can circulate with more or less resistance through the whole of the terrestrial mass and will only cease when, according to the law of the indestructibility of forces, it is changed into another force which manifests itself by thermal, chemical, or mechanical effects. Becquerel explains by electricity many phenomena of cementation which are observed in nature; and if one compares his simple and reasonable theory with that proposed by Plattner as an explanation of the formation of 'kernels,' when calcining certain cupriferous minerals, it is not possible to doubt about the real cause which produces 'kernels' containing 40 per cent of copper in the calcination of the Rio Tinto ore, of which even the richest portion seldom reaches 8 per cent. The electro-telluric actions may have played an important part in a number of geological phenomena, in the formation of the nodules and bands of flint in the Chalk, differently explained by Gaudry, Hébert, and d'Orbigny; the grains of oxide of iron in the Tertiary; the oolitic iron-ore in the Jurassic deposits; the spherosiderite of the Carboniferous formation and the geodic iron ore of the modern formation; the kidneys of phosphorite, which appear in distinct geological epochs; further, in the concentrical structure of certain limestone rocks and calcareous minerals; the tendency of rocks to assume a spherical form; the curious formation of embossed pebbles; the eaglestones, septaria, and other phenomena, during a considerable time considered merely as *lusus naturæ;* the formation of agates, the delicate layers

of which have a regularity and symmetry in their grouping which it is impossible to attribute to successive infiltrations through an orifice, merely imaginary in most cases. Electro-telluric actions play a considerable part in the formation of the concretionary lodes, which, according to De la Beche, can only be considered contemporaneous with the rocks in which they occur. The same remark applies, also, to the segregated lodes which Weissembach and English geologists explain as a grouping of the particular minerals disseminated in the containing rocks. Admitting that the metals and other substances which fill the rents and fissures in the earth's crust, and also the waters which have carried them, do not exclusively originate from the interior of the earth, but from the whole of the surrounding mass, from the surface to the lowest part of the rent or fissure; and further, that by the slow and successive entrance of these substances as well as by the circulation of the waters, and other previously mentioned causes, electrotelluric currents have been produced, all the phenomena of metamorphism, as regards the lodes, can be explained, and thus the greatest difficulties which this arduous problem presents can be made to disappear. The comparative greater richness of lodes running across the stratification than of those parallel to it, and also the well-known fact of an increased richness at the crossing of two lodes, can be explained by a relative greater display of electrotelluric action.

The reason why all the phenomena are attributed to the electrical force, which until now have been explained by molecular action, electrical repulsion, chemical affinity, and other causes, is the author's belief in the unity of the various natural forces. If physicists of great distinction have now arrived at the conclusion to attribute the light and heat emanating from the sun to continuous electrical discharges in his photosphere, as well as in his interior mass, one can well ascribe to electricity the actions which take place in the metamorphism of rocks, instead of considering it only, according to some geologists, as a secondary force, because heat, pressure, and other modes of force, produce electricity. Electricity, by itself, can not only originate these, but also other manifestations of molecular motion. In the same manner as the immense number of generations of infusorial animals have succeeded in building up monuments in rock, in comparison with which the grandest human constructions are mere playthings, the almost imperceptible electrical currents which exist in the earth's shell are able, by their number and continuity, to produce changes which neither man is able to bring forth, even when aided by the most powerful resources of science, nor nature herself could exceed, should she prefer to give us proofs of her power by changing the face of the earth by means of terrible cataclysms.

SUNSPOTS AND BRITISH WEATHER.

By W. L. DALLAS, of the Meteorological Office.

THE forecasts which appear in our daily papers refer only to a space of about twelve hours after their time of issue, and if they give us a correct idea, which as a rule they do, of the weather of the approaching day, they do all that was ever claimed for them, and their object is attained. Whatever may be the value of such information (and during harvesting operations it must be enormous), no one would probably deny that it is of much more importance to be able to give warning of a dry or wet season, than merely of a dry or wet day. When the seasons have their normal amounts of rain, cloud, and sunshine, the present system of forecasts may be all that is required, but when we are passing through such a spring and summer as was experienced last year, it is felt that something more is wanted, and the necessity of seasonal forecasts is fully recognized. The middle six months of 1879 were certainly unexampled for many years past for coldness and continuance of rain, yet we had no previous warning that such would be the case, and practical meteorologists were quite unable to say whether the showery, unsettled weather, when it had once set in, would last for only one month, or for two, or till the end of the year. In consequence, the capital of our agricultural community was as thoroughly lost as though it had been thrown into the sea. Hundreds of thousands of pounds that might have been profitably employed in some other way, or at least might have been laid by for a more favourable season, were expended in labour that was to prove quite abortive, and in seed that was destined to be washed away. It is now quite evident that while telegraphic reports of the actual state of atmospheric pressure, the winds, &c., may and do enable us to form a very correct estimate of the probable weather for several hours in advance, their indications are unsafe guides for any longer period, so that it becomes clear, that in order to succeed in forecasts

for, say, three or six months in advance, meteorologists must succeed in proving a connexion between our weather and some extra-terrestrial or other phenomenon which, being regular and periodic in its variations, would give us the necessary warning of approaching climatic change.

The apparent success attending the work of the Sunspot theorists, induced me to take a 'Sunspot period,' and by carefully working through it, having regard to the meteorological elements of rainfall, temperature, and weather, to find out whether some parallelism might not be discovered between the number of Sunspots and the weather experienced. Everyone is aware that nearly every important climatic alteration is due, directly or indirectly, to cosmical changes; and the first efforts of the advocates of the Spot theory having apparently been crowned with success, droughts, floods, and more recently commercial crises, have been ascribed to the presence or absence of spots on the surface of the Sun.

It is now matter of everyday knowledge, from the investigations of Wolf, Balfour Stewart, and Lockyer, that the number of spots which appear on the surface of the Sun has a regular period of increment and decrement, and that these changes proceed during a cycle of about 11·1 years. For long these spots were believed to be cooled solid bodies, or even clouds sailing about on the surface of the Sun, but it is now supposed that they are vortices in the surrounding atmosphere, caused by the descent of upper and cold currents into the interior of the Sun. Their numbers consequently indicate increased activity in the Sun's atmosphere, and hence, probably in solar radiation; and as all changes in our atmosphere depend on the intensity of solar radiation, the importance of their indications cannot be too highly estimated. It happens, unfortunately, that considerable divergences of opinion exist among the authorities who have investigated this matter. Some, among whom are Messrs. Baxendell and Blanford, maintain that solar radiation is much more intense in years of Sunspot maximum than in those of minimum, believing, in fact, that the spots themselves are indications of the intensity of solar radiation; while other writers, relying on the observations of Dr. Köppen, believe that while the electrical phenomena of our atmosphere are in some manner intensified according to the abundance of Sunspots, periods of high air temperature correspond rather with years of minimum than of maximum spots. A decision on this important point would assist considerably in all discussions as to the effect of these cosmical changes on the meteorology of the Earth; but until the establishment of observatories, in such almost cloudless districts as some parts of Africa, Arabia, or India, so that actinometrical observations can be continued for long periods under almost unvarying

conditions, it is difficult to believe that the subject can be satisfactorily settled. This uncertainty, however, does not much affect the present paper, the object of which is, not to find out either the origin or formation of the spots, but to ascertain whether the presence of greater or less numbers of spots on the surface of the Sun affects our atmosphere in such a manner as to cause a more or less marked periodicity in our climatic changes.

In Ceylon, the variation of the rainfall, and consequently of the cereals, in direct ratio with the spots, has become a matter of popular observation; but so far as we know, this is the only part of the Earth's surface in which the variation is so noticeable. Lying as Ceylon does, almost under the equator, it is of course there that we should expect to find any changes occasioned by alterations in the amount of solar radiation exceedingly strongly marked, and that this is the case is a great argument in favour of the upholders of the Sunspot theory. In Mr. Blanford's *Vade Mecum*, where rainfall and Sunspots are investigated with a minuteness commensurate with the importance of the subject to the Indian community, the rainfall of a long series of years is given for six important Indian stations. This series is subdivided into eleven-year periods, and the average rainfall of each of the homonymous years being taken, the difference of each average year is shown as a percentage of the local mean of each station, and in a second table as a percentage of the mean rainfall of all the six stations. The latter table, more particularly when the amounts have been smoothed down by a process which will be explained fully later, shows a very distinct variation in the amount of rain, a decided excess in years of Sunspot maximum, and a decided deficit in those of Sunspot minimum. The following figures, copied from the *Vade Mecum*, show the rainfall for Madras, and it is impossible to deny that there is very strong evidence of a connexion between the two phenomena of precipitation of rain and of the presence or absence of spots on the surface of the Sun:—

Year.	Inch.
1860, &c.	+ 8·5 per cent.
1861 „	+ 6·2 „
1862 „	+ 9·2 „
1863 „	+ 5·8 „
1864 „	− 7·1 „
1865 „	− 13·2 „
1866 „	− 12·7 „
1867 „	− 14·0 „
1868 „	− 6·0 „
1869 „	+ 6·0 „
1870, &c.	+ 11·7 „

The group of years under 1866 being that of average Sunspot minimum, and that under 1870 being that of Sunspot maximum, the above figures show a very distinct variation in accordance with the relative number of Sunspots, a difference of 25 per cent existing between the rainfall of the series of years of maximum spots, and that of the series of minimum spots. While, however, quoting this table, which apparently shows incontestable evidence of a parallelism between rainfall and Sunspots, it must be borne in mind that the returns are for one station only, and for a station which, just as in the case of Ceylon, must, from its geographical position, be peculiarly susceptible of any changes in the direct action of the Sun. In other places, even in India, no such marked cyclical variation is shown; and it is remarkable that one of the most severe famines which has been experienced in Northern India, occurred in 1837–8, and was directly attributable to scarcity of rain, during two years which were those of maximum Sunspots.

With such incongruities in India, what was to be hoped for in the British Isles? Any cyclical variation, as well as our smaller atmospheric changes, all depend for their motive force on solar radiation. The force of the wind and its direction, the rain and the fine weather, all depend directly or indirectly on the amount of heat we receive from the Sun; and when this is remembered, it will be understood how variations, which may even be strongly marked under the equator, are hardly noticeable in the north-west of Europe. In dealing with different zones of the Earth's surface, however, it must be borne in mind that the very same causes which may occasion certain changes in the tropical regions may exert an influence in a directly opposite direction in the temperate zones, so that to look for an exact parallelism between the secular changes in the tropics and those in other parts of the Earth's surface, is to disregard entirely the enormous physical differences which exist between those respective regions, and in fact ignore the heat of the Sun, the prime mover of all the variations in our atmosphere. Turning back to the Madras register, it is remarkable that the year of minimum rainfall is not the same as that of minimum Sunspots, but occurs one year later. If then, at Madras, it is found that the cycle of rainfall is one year later than that of Sunspots, we, in the temperate zone, might be prepared for a much greater discrepancy; the experience of the tropics might even be reversed in England, and yet the theory of the advocates of the Sunspot school remain practically unshaken. Let it be supposed that the heat radiated from the Sun is much less in years of Sunspot maxima than in those of Sunspot minima, it is then fair to assume that the general wind over the surface of the Earth would be less strong, so that in consequence

a large amount of precipitation would occur in the tropics, the enormous amount of water which is daily evaporated there not being carried quickly away to other regions. Such a hypothesis would satisfactorily explain an excess of rain in the tropics, and a deficit in the temperate zone in the same year, a condition of affairs, which, without some such explanation, would be fatal to the Sunspot theory. From the works of some of the most eminent of the Sunspot theorists, however, it appeared that any such argument was quite unnecessary, the rainfall of different stations, collected from independent sources, displaying an apparently exact agreement with the number of spots. Taking Edinburgh, we find that the figures quoted by Mr. Meldrum show a remarkable parallelism between the number of spots and the amount of rain. The period under discussion extends from 1824 to 1867, and each year's rainfall has been classified according to its position in the series as a year of maximum, minimum, or intermediate Sunspots, so that the variation from the mean rainfall of all the years of maximum Sunspots is shown by the figures in the fifth year of the series, and the variation of all the years of Sunspot minima by the figures in the first or last lines:—

Rainfall. Variation from the Mean. Inches.	Sunspots. Variation from the Mean.
− 2·8	− 37·2
− 1·8	− 22·8
+ 0·7	+ 4·4
+ 2·4	+ 33·0
+ 3·3	+ 43·8
+ 2·8	+ 32·9
+ 0·5	+ 14·3
− 0·4	− 2·9
− 1·0	− 16·6
− 2·5	− 24·7
− 1·7	− 24·0

From this table it was hoped that the general rainfall of the British Isles might be found to vary regularly according to the increment and decrement of the Sunspots; but it is never safe to take the rainfall of any one station, and conclude that it properly represents a large district, and gives the normal rainfall for that part of the Earth's surface. It happens, however, that the Meteorological Office has lately issued a small pamphlet, in the form of a supplement to the Weekly Weather Report, in which are given the best available averages for fourteen years of temperature and rainfall, not for particular stations, but for the different meteorological districts into which the British Islands have been divided. By accepting these tables, we, in a great

measure, do away with local peculiarities, and have a fair idea presented of the general rainfall and temperature over the kingdom. The eleven-year cycle, which we find embraced by the fourteen years quoted in the pamphlet, runs from 1867 to 1877, and the following is the relative number of Sunspots according to Wolff in this period:—

1867	8·8	per cent.
1868	36·8	„
1869	78·6	„
1870	131·8	„
1871	113·8	„
1872	99·7	„
1873	67·7	„
1874	43·1	„
1875	18·9	„
1876	11·3	„
1877	7·0	„

It will be noticed that the relative number of Sunspots rises from a very small number in 1867 to a very considerable one in 1870, and then decreases again to about the original figure by 1877. The increase is, however, much more rapid than the decrease, and it has been proved by Mr. Meldrum that the epoch of maximum number of Sunspots occurs on the average about four years after the epoch of minimum, and the epoch of minimum about seven years after the epoch of maximum number. So that though the cycle is one of eleven years, yet that period is not divided into two equal parts, during one of which a regular annual increment occurs, and during the other an equally regular decrement; but into a period of four years of increment and seven of decrement. In the cycle under discussion, the epochs of maximum and minimum number of Sunspots are fortunately more strongly marked than they have been at any time during the past fifty years, so that if the actual number of Sunspots have any decided effect on our weather, the results should be more strongly accentuated during these years than at any other time. The following figures show the mean rainfall of the British Isles, divided into eastern and western divisions, and for the Islands as a whole:—

	East.	West.	British Isles generally.
1866	34·8	46·9	40·3
1867	31·2	41·0	36·0
1868	33·2	44·4	38·3
1869	32·5	42·4	37·0
1870	25·6	35·3	30·0
1871	29·4	39·7	34·1
1872	41·8	58·0	49·2

	East.	West.	British Isles generally.
1873	29·6	39·3	34·0
1874	29·5	41·4	34·9
1875	32·9	42·3	37·2
1876	34·5	44·1	38·9
1877	37·2	52·7	44·2
1878	29·9	39·1	34·1

It will be noticed that the years 1866 and 1878 are quoted in the above list, as, by taking these two years, we are enabled to make use of a smoothing process, by means of which the rainfall is welded, as it were, into one continuous band. In drawing the curve *a*, in fig. 1, the following plan has been adopted: Let *a*, *b*, *c*, &c. equal the first, second, third, &c. annual amounts, then $\frac{a+2b+c}{4}$ gives the amount of rainfall which is taken to represent the rainfall of the first year of the curve, and so on throughout the whole series. In the second curve (*b*) the actual amount for each of the years is given under its respective date, and no attempt at smoothing down is made, but it will be noticed that the two curves show a very close resemblance to one another, the difference being only in degree:—

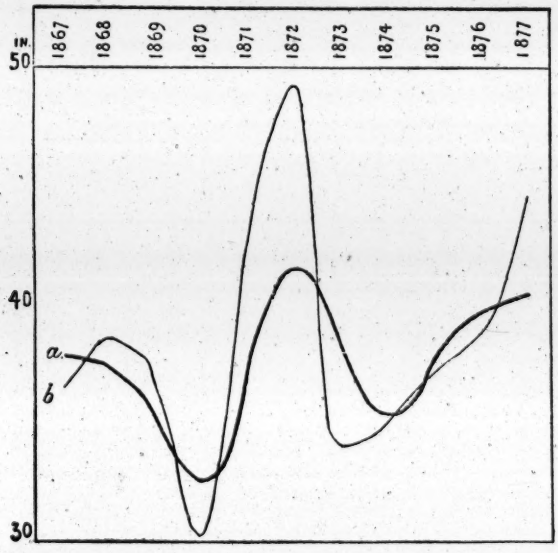


FIG. 1. Curves showing the Annual Rainfall in the years 1867–1877 inclusive.

From these curves it will be seen that so far from the year of maximum rainfall being that of maximum Sunspots, the reverse is the case, the year 1870, which has an exceptionally large relative number of Sunspots, showing the smallest amount

of rainfall. This might be explained by the hypothesis, proposed above, but supposing such to be the case, then the years 1867 and 1877 should be years of considerable rainfall. This, however, is far from being the case, the years at the extremity of our cycle showing only a very slight excess over the mean rainfall for the period. The year 1872, however, which is only two years after the year of maximum Sunspots, and should, according to the above reasoning, show a pronounced deficit, has the heaviest fall of any year during the whole period, the total amount of rainfall exceeding by nineteen inches that which was collected in 1870. It will also be noticed that both 1867 and 1877 are years of Sunspot minima. If then we take 1869 and 1879, and discuss their rainfall, we are taking two years which are distinctly comparable, their positions in the eleven-year cycle being exactly the same. It will, however, be found that fifty per cent more rain fell in the six months April to September, 1879, than fell in the same six months of the year 1869, so that a prophecy, based on the rainfall of 1869, could have given us no warning of the amount of rain with which we were to be visited in 1879. The following table gives (1) the actual amounts of rainfall measured within the British Isles during the eleven years under discussion; (2) the amounts smoothed down by the above-mentioned process; (3) the difference of each year shown as a percentage of the mean rainfall; and (4) the relative number (also smoothed down) of Sunspots for each year:—

	In.	In.	Per-centages.	Sunspots.
1867	36·0	37·7	+ 0·3	8·8
1868	38·3	37·4	− 0·5	36·8
1869	37·0	35·6	− 5·3	78·6
1870	30·0	32·8	− 12·8	131·8
1871	34·1	36·9	− 1·9	113·8
1872	49·2	41·2	+ 16·4	99·7
1873	34·0	38·0	+ 1·1	67·7
1874	34·9	35·3	− 6·2	43·1
1875	37·2	37·1	− 1·3	18·9
1876	38·9	39·8	+ 5·8	11·3
1877	44·2	40·4	+ 7·4	7·0

Leaving rainfall and turning to temperature, the same process has been adopted with the mean temperatures of the eleven years as that adopted with amounts of rainfall, but the mean temperature shows a course as irregular as that of rainfall, nor do the two together show any peculiarities in common (see fig. 2).

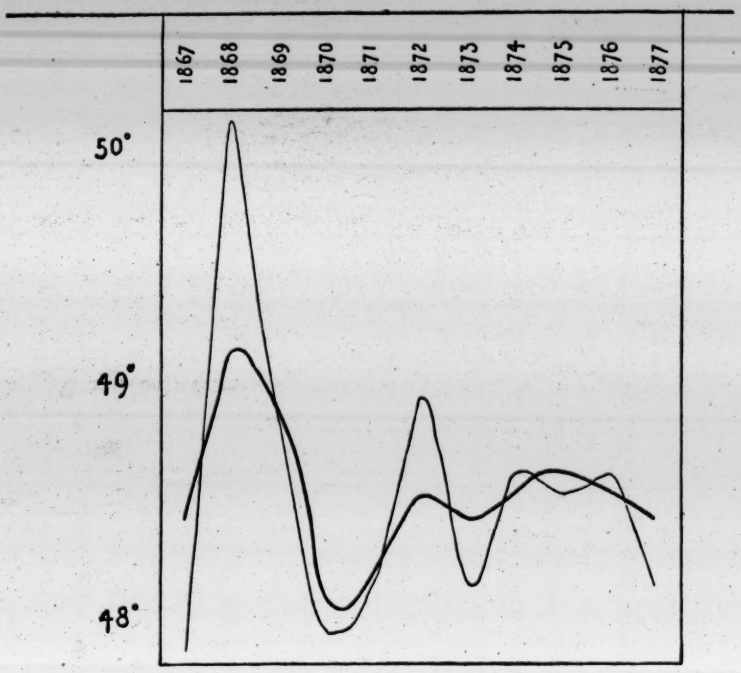


FIG. 2. Curves showing Annual temperatures in the years 1867–1877 inclusive. As in fig. 1, the darker line is the smoothed curve.

Whatever may be the result of the actinometrical observations which, it is to be hoped, may one day be made in one of the almost cloudless districts of the world, it certainly appears that there is no concord whatever between the relative number of Sunspots and the heat radiated on the British Isles. The hottest year of the whole series was 1868, its six central months being quite exceptionally warm, but if we attempt to connect that fact in any way with Sunspots, we find that it was the first year after a year of minimum Sunspots; but on passing on eleven years, and taking the first year following the next Sunspot minimum, we find that it was in no way remarkable for excessive heat. Following on to the next year, that is to say, the second after the year of Sunspot minimum, we find 1869 with a temperature very near the mean; but on again passing on eleven years, we come to 1879, which was considerably the coldest year that has been experienced for a very long time.

Taking up the question of the heat of the summer months of the different years, we are unable to derive any great comfort or hope from an examination of their mean temperatures. It would have been fair to expect that in a curve of the mean summer temperatures, some sort of evidence of the existence of a cycle might be traced, even though in a curve of the temperatures of the whole year its existence might be hidden by the

readings taken in the winter. This, however, is not apparently the case, the differences which do occur seemingly following no rule. The summer temperature of 1867 was decidedly low; but that of 1868 was decidedly high; from 1868 until 1873 the summers, with slight oscillations, became colder and colder; from 1873 to 1875 they became slightly warmer again; but, with 1876 a fall set in, and the summer of 1877 was the coldest we have experienced for many years past.

The winter means display equally inexplicable irregularities. The years 1868, 1869, 1872, 1874, and 1877, enjoying more or less mild winters, and the years 1867, 1870, 1873, and 1876, experiencing, more or less, severe weather. In the years 1867 and 1877, which are both years of Sunspot minimum, it will be noticed great differences exist in the winters; that of 1867 is the coldest of the whole period, whilst that of 1877 is among the warmer ones. The following curves (fig. 3) give the actual mean temperatures of the summers and winters of this period. The first curve with the scale on the left gives the winter, and the second curve with the scale on the right the summer means.

In conclusion, it has been attempted to connect the electrical phenomena of our atmosphere with solar physics; but with the same unfortunate result. The average number of days on which thunderstorms occurred during each summer of the period was forty-nine; and the following table shows, for each year, the departure from that mean :—

In 1867	− 5	days.
1868	+ 4	„
1869	− 5	„
1870	− 16	„
1871	+ 12	„
1872	+ 20	„
1873	− 6	„
1874	0	„
1875	+ 5	„
1876	− 6	„
1877	− 3	„

These figures, like the former, give no evidence of any variation, either with or against the number of Sunspots. Both 1867 and 1877, the years of Sunspot minimum, show a slight deficit in the number of days with thunder; but on the other hand, 1870, which is the year of maximum Sunspots, shows a much more considerable falling off, while 1871 and 1872 have a very large excess.

With the whole mass of evidence before us then, it becomes almost imperative to confess that, as practical guides in fore-

casting weather, Sunspots must remain of very secondary importance. It must be remembered, that science, like politics, has a practical and a purely theoretical side; and that while

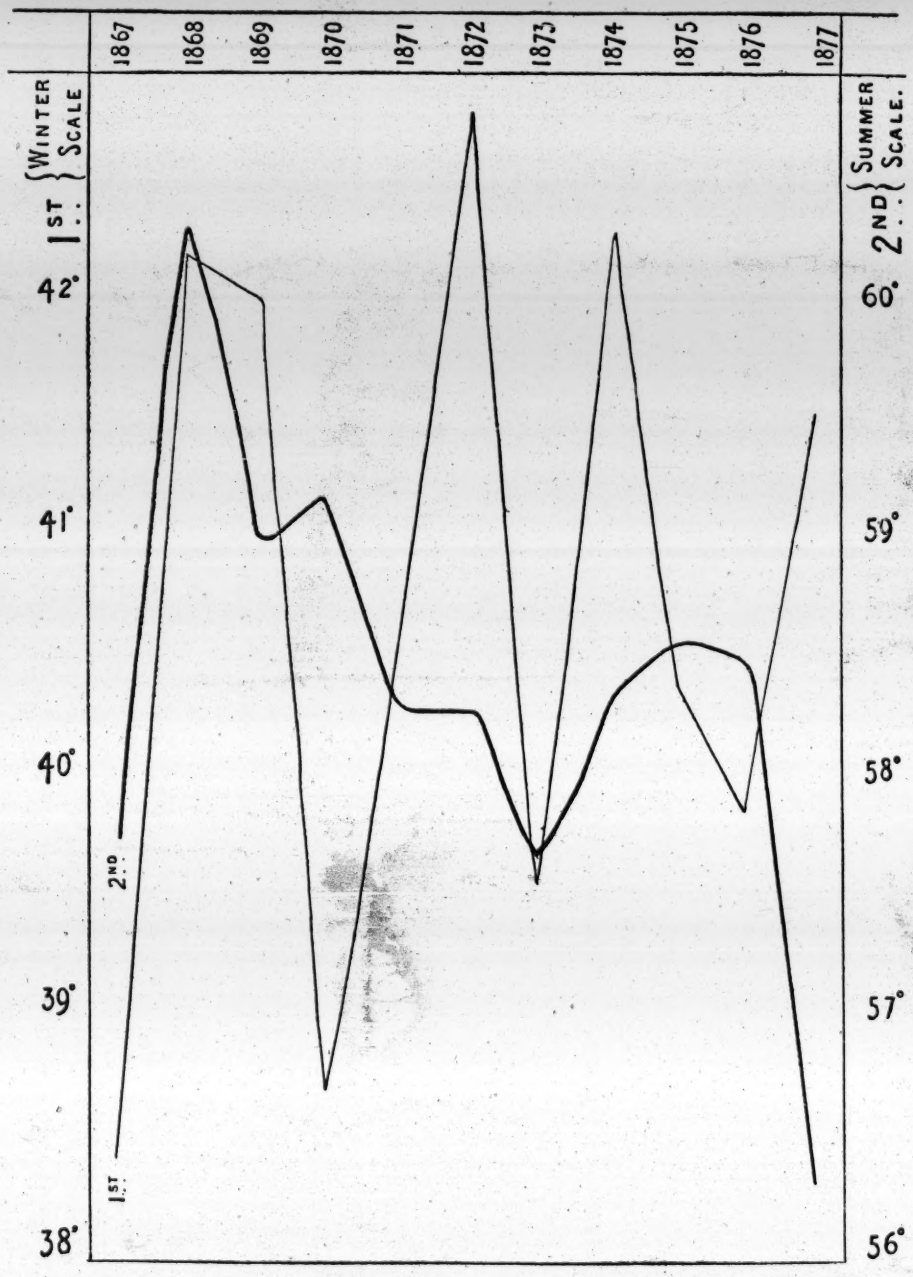


FIG. 3. Curves showing Summer and Winter temperatures in the years 1867–1877 inclusive.

evidence of the correctness of a theory, even if it be of the very slightest character, is a reward amply sufficient to the theoretical student, proof that the theory is of practical importance is

required to arouse the enthusiasm of another class of workers. From a practical point of view, Sunspots seem to be a failure. It is undeniable, from the evidence of the Edinburgh Return quoted above, that taken through a long series of years, the rainfall apparently increases and decreases with the number of spots on the surface of the Sun ; but in the cycle of eleven years we have had under discussion, the variation in the number of spots was very exceptionally marked, and during the period exceptionally hot and exceptionally wet years occurred, yet in neither case was there any apparent connection between these abnormal conditions and the state of solar physics. It follows, then, that if it is necessary to have averages extending over half a century in order to be able to show even a slight variation of the rainfall with the number of Sunspots, and if, even then, it is possible to have an exceedingly dry year at the epoch of maximum Sunspots, it is evident that to undertake, at any particular time, important operations dependent on the weather for their success, on account of the indications of the solar cycle, would be running the risk of almost certain disappointment. Whether further investigations of astronomers will disclose other cycles than that of eleven years, it is impossible to say, but any cosmical or other changes which may assist meteorologists in their hitherto vain attempts to forecast approaching seasons will be welcomed with enthusiasm, and will be sure of an attentive and favourable investigation. That the approach of such wet seasons, as it was our fate to pass through last year, will one day be foretold in ample time to take every necessary precaution, I feel sure; but that this timely warning will not be given by considerations of the year's position in an eleven-year cycle of solar spots, I am reluctantly compelled to believe.

THE HARDENING AND TEMPERING OF STEEL.

AN announcement was made some time ago that the Institution of Mechanical Engineers had resolved to devote a sum of money to experimental research on mechanical questions. The first-fruits of this resolution have since appeared in the shape of a First Report on each of the three subjects selected by the Council for examination, viz., the hardening, tempering, and annealing of steel; the best form of riveted joints; and friction between solid bodies at high velocities.

It will be seen that these subjects are all of great importance, and that two at least—the first and the last—possess a scientific as well as a practical interest. The reports upon them do not describe any new experiments actually undertaken by the Institution. They are preliminary and historical; giving a digest of the theories that have been broached, and the experiments that have been made, by former labourers in each subject, and going on to point out wherein these are imperfect, and what remains to be done in order to achieve in each case the final solution of the problem. This historical element is itself of much interest, as showing the mode in which such questions have been attacked, both from the theoretical and the practical side; and we regret that our limits do not allow us to reproduce the reports *in extenso*. We must, however, content ourselves at present with a short *résumé* of the first subject on the list, viz., the hardening, tempering, and annealing of steel; pointing out first its essential features, and then noticing the light which is thrown upon it by the report in question.

The facts relating to this subject are extremely well known, and are continually acted upon; their importance is almost unique. Probably there is hardly a known process, scientific, surgical, mechanical, or industrial, which does not at some stage depend for its success on the use of properly tempered steel tools. It seems strange, therefore, that the theory of the subject should be in a state of the utmost uncertainty and confusion; and that even the direct experiments

made to elucidate it should have been very few. We believe we are right in saying that for some years the subject had the joint attention of two such minds as those of Faraday and James Nasmyth; but the results obtained were not considered sufficiently important to warrant their publication. It will be seen, therefore, that the question is eminently worthy of thorough scientific examination.

We will first state, as briefly as possible, the main facts of the case. We shall confine ourselves to 'hard' steel, such as is used for tools, containing not less than say $\frac{1}{2}$ per cent of carbon; with 'mild' steel, containing a smaller quantity, the phenomena are different. Hard steel, as produced mainly by the cementation process, can be hardened by being raised to a high temperature, and then suddenly cooled, or 'quenched,' in a bath of some cold fluid, generally water or oil. The outside is always the hardest, but, if the thickness be moderate, there is considerable hardening throughout. The colder the bath, the greater the hardness of the steel, until it reaches the 'ice-brook's temper' of Othello's sword; but steel in passing rapidly through so long a range of cooling is very apt to 'fly,' or become brittle. For tough steel, therefore, the range of cooling should be as low as possible. By cooling in oil, the tenacity of the steel is supposed to be increased; and this increase is believed to be greater the higher the temperature of quenching: but the difference between water-cooling and oil-cooling is probably only due to the fact that the latter is a more gradual process. Unless the initial temperature of the steel is above a certain point, the hardening does not take place at all.

It will be seen that the phenomena vary materially with the initial and final temperatures of the steel, and to some extent with the rapidity of its passage from one to the other. It will also be seen that the essential qualities of hardness and toughness are opposed to each other; in other words, the steel will be more hard, but at the same time less tough, as the fall of temperature is greater and more rapid. Hence it is of the utmost importance that the cooling should be as gradual as is consistent with giving to the steel that degree of hardness which is needful for the work it has to do; it will then be as tough as it can be made to be sufficiently hard. In the bringing of any article to this desirable condition consists the process of tempering. In practice this is effected by first heating and quenching the steel, so as to harden it in excess, then raising it again very carefully to a certain temperature, varying according to the use for which it is intended, and then quenching it again from that temperature. The manner in which the correct temperature is ascertained is very curious and striking. Take the case of an ordinary cold chisel, which is usually hardened

and tempered at one operation. The smith first heats it in his fire, protecting it from the atmosphere by laying it in the small coal. When he judges the heat sufficient, he draws it out, and dips the point in cold water, thus producing a rapid cooling and hardening. As soon as the cooling is completed, he lifts the point from the water, polishes it slightly, if necessary, with a grindstone or a file, and then watches it narrowly. The heat in the main body of the tool communicates itself to the point, and as the temperature of the bright surface rises, its original white lustre is seen to alter rapidly, first to a pale yellow, then to a straw colour, then to a full yellow, then to a brownish orange. As soon as this last tint appears, the smith drops the point again into the water, in full confidence that when cooled it will be of the right temper for its work. If, however, the heating were continued, the brown would become dappled with purple, and would then be succeeded by a full purple, light blue, full blue, and dark blue, in regular order; and each of these would mark a point at which the steel should be quenched to give the temper proper for some particular tool; *e.g.*, bright blue for swords and watch-springs, dark blue for saws.

Such being the facts, let us see how far theory has been able to account for them. This is what the Committee's Report has to tell us. Unfortunately, it is professedly deficient in its account of the German literature on the subject, owing, it is said, to the difficulty of obtaining good information from that country. This deficiency, we believe, is to be supplied in a revised edition. Meantime, we can at least deal with the French and English views of the question. We should premise that the Report has been primarily the work of Mr. W. Anderson, of the firm of Easton and Anderson, well known for his translation of Chernoff's important papers on steel; but the Committee also comprises such names as those of Chernoff himself, Professor Williamson, Professor F. A. Abel, Mr. J. Vavasseur, and others.

I. *Composition of Steel and Cast Iron.*—The early view seems to have been that the carbon in steel or cast-iron formed, at any rate in part, some definite chemical combination with the iron; but about 1852, Jullien developed the theory that the carbon was always present as a 'solution' merely—liquid when the iron was hot, solid when it was cold. The term 'solution,' as applied to a solid substance, has not been fully accepted by English chemists; but it appears to have a real signification, as expressing a mixture so intimate that the smallest particle of the mixture which can be isolated will always be found to contain both components. In some cases, as in very grey cast-iron, the carbon is not even wholly in solution, but partly exists in specks, more or less large, of pure graphite. In this state it

has generally been called 'free' carbon, and when in solution 'combined' carbon. As to the proportion in which these two forms occur in various qualities of iron and steel, Åkerman, Barba, and others, have advanced various opinions; but the view that there exists in steel or cast-iron any definite chemical combination, or carburet of iron, appears to be abandoned on all sides. It seems generally allowed that no other elements than carbon and iron should be present in really pure steel; and that other substances, such as manganese, which are generally considered to improve steel, only do so by neutralizing the action of other and more hurtful impurities.

II. *Condition of the Carbon of Iron in Steel.*—Jullien's view on this part of the subject is, that molten cast-iron is a solution of liquid carbon in liquid iron: that when the metal is cooled slowly, part of the carbon separates out as pure graphite, while the remainder continues in solution, thus forming 'grey,' or soft cast-iron; but that, when the metal is cooled quickly, this separation does not occur, and that the result is a solution of crystallized carbon in amorphous iron, forming 'white,' or hard cast-iron. Exactly the same process occurs with steel, and makes the difference between hard and mild steel. Caron and Åkerman generally confirm this view. They point out that hardened steel dissolves in hydrochloric acid without leaving any residue; but that the same steel, if first annealed, by being kept a long time at a red heat and allowed to cool slowly, leaves a residue of carbon insoluble in the acid. This seems to show that there is a more intimate mixture between the two elements in the former case, and that the carbon either dissolves with the iron, or, more probably, escapes as carburetted hydrogen.

III. *Hardening of Steel.*—On this, the central point of the whole inquiry, Jullien advances a very bold and original theory. He holds that carbon, in contact with red-hot iron, becomes liquid and is absorbed like water in a sponge; that, if cooled slowly, the carbon becomes amorphous, and the steel is soft; but if cooled quickly, the carbon crystallizes, taking the properties of diamond, and the steel becomes, in fact, diamond set in iron. The hardness of the steel is thus simply due to the hardness of the crystallized carbon. In support of this, he remarks that all hard bodies take different molecular structures, according as they are cooled rapidly or slowly; *e.g.*, gold is fibrous when cast in a metal mould, crystalline when cast in a sand mould; glass is crystalline when cooled rapidly, but amorphous when annealed. He further observes that diamond, heated for a long time in a closed vessel, becomes graphite, and hence concludes that liquid carbon, rapidly cooled, would become diamond.

This theory, striking as it is, is beset with difficulties. As

the Report observes, it is difficult to see how it can possibly account for tempering, *i.e.*, for the nice gradations of hardness, varying with each small variation of temperature, and showing each its characteristic colour, which have been described above. Moreover, as there is no known process of liquefying carbon, it is hard to believe that it is effected by the mere presence of hot air without the occurrence of any chemical reaction.

Barba and Åkerman advance a theory altogether different, viz., that the severe compression in the several layers, produced by their contraction during rapid cooling, retains a greater proportion of carbon in solution, instead of allowing it to separate out as graphite. Åkerman supposes that the compression itself makes the metal more compact, and therefore harder, exactly as it is hardened in cold-rolling and wire-drawing. But, as the Report observes, the outer layers, which cool first, are brought into a state of tension, not compression, by their efforts to contract over the still heated core; and yet it is precisely these outer layers which attain the greatest degree of hardness. There is, in fact, no reason to think that a piece of metal (*e.g.*, a thin sheet of steel) heated and then cooled uniformly, would suffer any internal compression at all; yet it would undoubtedly be hardened.

The exact molecular changes that occur during the heating and cooling of steel were ably discussed by Chernoff in 1868. His view is, that there is a certain temperature, *a*, which must be overpassed before any hardening effect can be produced by cooling; and a higher temperature, *b*, at which steel takes an amorphous, wax-like form, and on cooling from which it crystallizes into large crystals if the process is slow and undisturbed, but into small crystals if the process is rapid or disturbed by hammering. Now for toughness and uniformity, in almost any metal, fine regular grain is essential; hence steel is improved, while at the same time hardened, by heating above temperature *b*, and then cooling rapidly. If it is again heated, it begins to soften, without losing its quality; but, so long as the heat is below temperature *a*, any required degree of softness can be permanently fixed, by simply quenching the steel as soon as it has reached the proper point, as indicated by the characteristic colour. The phenomena of tempering are thus in some measure explained; but those of hardening still remain a mystery, since, although large crystals are unfavourable to the toughness, there is no reason to suppose that they affect the hardness of a metal. The existence, however, of some such molecular changes as Chernoff describes is confirmed by many other facts, *e.g.*, the sudden and temporary expansions observed during the cooling of iron wire by Gore and Norris.

Dissatisfied with the theories of their predecessors, the

Committee have boldly struck out an idea of their own. This has been suggested by Edison's experiments on platinum wire, communicated to the American Association for the Advancement of Science at Saratoga in 1879. These experiments showed that incandescent platinum wire became covered with minute fissures, due to the expiration of the occluded gases under the action of the heat: and that when the wire was allowed to cool *in vacuo*, these cracks closed up again and disappeared. By a succession of heatings and coolings *in vacuo*, the whole of the occluded gases were expelled, and the metal was then greatly altered in character, becoming much more dense and hard, and remaining perfectly rigid under the most intense incandescence. The Committee suggest that the same action may take place in steel; that the heating of the metal expels the gases which remain occluded at ordinary temperatures, and that the sudden contraction in rapid cooling prevents their re-absorption (perhaps actually assists their expulsion); the particles of the metal are thus brought closer together, and their force of cohesion is increased. When the metal is gently heated, as in tempering, re-absorption begins; and the characteristic colours are due to changes in the surface (*e.g.*, the gradual opening of minute fissures) which are produced by this re-absorption.

We do not propose at present to criticize this theory. The Committee suggest the carrying out of a series of experiments, with the special view of testing its truth. But since the Report was published, the subject has been discussed at a General Meeting of the Institution, and Prof. D. E. Hughes then gave it as his opinion, supported by recent researches, that soft steel was a mere coarse mixture of iron and carbon, while in hardened steel part of the carbon at least was in the form of an actual alloy with the iron, and that the qualities of the steel depend upon the constitution of this alloy. This was illustrated by the fact that hard steel was readily attacked by dilute sulphuric acid, while soft steel was not, which he accounted for by supposing that in the former case the combination was so intimate, that local galvanic circuits were set up, each molecule forming a minute carbon and iron battery. It is to be hoped that this theory, taken in conjunction with the opposing theory of occlusion, will be fully and carefully investigated, especially as such an investigation, whether it confirms either theory or not, can hardly fail to throw considerable light upon the very interesting phenomena we have here attempted to describe.

REVIEWS.

EARLY MAN IN BRITAIN.*

THOSE of our readers who have the good or ill fortune (which is it?) of having attained middle age, will remember the pictures of the ancient inhabitants of Britain which used to adorn the first page of the *History of England*. Nearly naked figures, adorned with an elegant tattooed pattern, and bearing skins on their shoulders as their sole possible protection from the inclemency of the weather, represented our primeval progenitors; and we were quite contented with the information that they drove war-chariots with scythe-blades attached to the axles, that their priests were the Druids, who venerated the mistletoe, and under whose directions Stonehenge was built, and that, among other amiable characteristics, they had that of offering human sacrifices. At that time, and even later, the learned, although they might dispute about Picts and Scots, Celtic migrations, Scandinavian invasions, and so forth, really knew little more about the matter than the boys at school. The *History of England* began with the ancient Britons, and that was pretty nearly all that could be said.

Prof. Boyd Dawkins would carry the initial chapter of this history much further back. For him the preliminaries of human history in any given locality may be considered to range over all time, and an acquaintance with Tertiary Geology at any rate is essential to the student of man. Thus at the outset of his *Early Man in Britain*, after indicating the close interrelation of the three sciences of Geology, Archæology, and History, he sets forth the general principles of Palæontological succession, and then discusses the principal phenomena of the Tertiary epoch, the members of which he classifies and characterizes by reference to the remains of mammalia contained in the respective deposits. This, of course, is essential to the discussion of the question whether man is a Tertiary animal, into which the author enters at considerable length. His conclusion with regard to the Eocene and Miocene periods is that as none of the mammalia which lived in those times are now in existence, 'It is unreasonable to suppose that man, the most highly specialized of all, should have been then on the earth,' an argu-

* *Early Man in Britain, and his place in the Tertiary Period.* By W. Boyd Dawkins, M.A., F.R.S. 8vo. London. Macmillan and Co. 1880.

ment of considerable weight, although not quite conclusive; especially when we consider that in order to do away with certain supposed evidences of man in Miocene times, Prof. Boyd Dawkins is obliged to 'suggest that they were made by one of the higher apes then living in France rather than by man,' the things to be accounted for being chipped flints and a notched fragment of a rib of *Halitherium*. The same argument from the paucity of still extant species of mammalia is urged in opposition to the existence of man in the Pliocene; and it is not until post-Pliocene times, when living species of mammals were already abundant, that man, 'as might have been expected,' indubitably makes his appearance on the stage. From this time there is not only no doubt of the existence of man properly so called, but the persevering researches of archæologists in this country and abroad have made us acquainted with a good many particulars as to the characters and habits of these early peoples, leading gradually up to that period when documentary history of greater or less value becomes available. It is to the development of this history of prehistoric man as learned by geological and archæological research, that Prof. Dawkins' most valuable and interesting book is devoted. Its title, namely, *Early Man in Britain*, is perhaps hardly sufficient to indicate the variety and importance of its contents, for besides that it gives an excellent sketch of the European Tertiaries as elucidated by the mammalian remains contained in their deposits, even in treating of man, its more immediate object, the author's attention has by no means been confined to the narrow limits of these islands, but in investigating the sources of the human population of Britain, he has, almost perforce, entered upon the consideration of the origin and ethnological relationships of the peoples inhabiting the whole of Europe. The author's line will be best explained by the following extract from his concluding summary:—

'The River-drift man,' he says, 'first comes before us, endowed with all human attributes, and without any signs of a closer alliance to the lower animals than is presented by the savages of to-day; as a hunter, armed with rude stone implements, living, not merely in Britain, but throughout western and southern Europe, northern Africa, Asia Minor, and India. Next follows the Cave-man, possessed of better implements, and endowed with the faculty of representing animal forms with extraordinary fidelity, living in Europe, north of the Alps and Pyrenees, as far as Derbyshire, and probably belonging to the same race as the Eskimos. The disappearance of the Cave-man from Britain coincided with the geographical change from a severe to a temperate climate, the extinction of some animals, and the retreat of others to northern and to southern regions. In the pre-historic age the earliest of the present inhabitants arrived in Britain. The small, dark, non-Aryan peoples, who spread over France and Spain, brought with them into Britain the domestic animals, and the cultivated plants and seeds, and laid the foundation of our present culture. The next invaders were the bronze-using Celtic tribes, composing the van of the Aryan race. They crossed over from the Continent, and introduced a higher civilization than that of the Neolithic age. In the course of time the use of iron became known, and in the Prehistoric Iron-age the condition of Britain was higher than it had ever been before.'

This summary of the principal theme of the volume now before us will suffice to show the amount of interest attaching to its contents. The

evidence bearing upon all these points is given very fully by Prof. Dawkins, who also deals in a similar spirit with a host of correlative details, tending to show the conditions which must have surrounded our early ancestors, and influenced them in their progress towards civilization. Nor does he forget the influence exerted upon the population of Britain, and, indeed, of western and northern Europe generally, by the more advanced peoples of the Mediterranean region. Thus his book furnishes an admirable picture of the history of man in this part of the world, prior to the existence of written documents, so far as it can be made out from the occasional glimmerings of evidence which shine upon us through the twilight of the distant past. We may add, in conclusion, that this word-picture is copiously and well illustrated with pictures of another sort, which place before the student the actual representations of many of the more important objects to which the author has occasion to refer.

A TEXT-BOOK OF BOTANY.*

NUMEROUS as are the extant manuals and text-books of Botany, English students of that science certainly owe a considerable debt of gratitude to Dr. Vines for the production of a translation of Dr. Prantl's text-book. The original work was built up on the same lines as the well-known treatise of Prof. Sachs, with the purpose of furnishing a smaller and less elaborate Manual of Botany than the *Lehrbuch*, and one which, while it might suffice for all the requirements of many students, would, at the same time, serve as an introduction to the larger work. We are not acquainted with the German original, but, so far as we can judge, the translation has been very carefully made, and the treatment of the subject is so concise and simple that the book cannot but exercise a most beneficial influence on those who use it as a guide in their botanical studies.

The author commences with a short chapter on the general morphology of plants, and then, in his two succeeding sections, treats of their minute anatomy and physiology. The special morphology of the various types of plants is relegated to the sections treating of classification, in which the structure and relations of the different parts of plants are described in some detail under the heads of the great divisions of the vegetable kingdom. This arrangement of the subject is very advantageous, and leads the student directly to a more philosophical conception of the subject than was generally attainable from the older manuals.

The section on classification, enlarged as above indicated, occupies more than two-thirds of the volume. The classification adopted differs in some particulars from the ordinary one in use in this country, especially in the wider limits of the orders adopted, which are, consequently, reduced in number, while some groups of plants are referred to somewhat different positions from those usually assigned to them by English writers. The translator has,

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* *An Elementary Text-Book of Botany.*—Translated from the German of Dr. K. Prantl, Professor of Botany in the Royal Academy of Forestry, Aschaffenburg, Bavaria. The Translation revised by S. H. Vines, M.A., D. Sc., F.L.S. 8vo. London: Sonnenschein & Allen, 1880.

however, given a consensus of the two classifications of Phanerogams, which will be of material service to the student. He has also substituted Prof. Sachs' classification of Thallophytes for that given by the author. We notice that the composite Algo-fungal nature of the Lichens is here accepted as proved. The woodcut illustrations are excellent throughout the whole book.

ZOOLOGICAL CLASSIFICATION.*

WE are very glad to see that Mr. Pascoe's little handbook of systematic Zoology, the first appearance of which we noticed in April, 1877, has been so successful as to cause him to bring out a second and greatly enlarged edition of it. The general plan of the work continues the same, but the author has modified the classification here and there in accordance with treatises published during the last four years, and he has added greatly to the statements of the opinions of different authors on the classification and value of various groups, thus making his book, which is still small enough to be a most convenient pocket-companion, an exceedingly valuable compendium of Zoology. The glossary of zoological, anatomical, and physiological terms, which the author has appended to the book, and which, in general, will be found useful, is certainly in some points not so good as it ought to be, some of the definitions being unsatisfactory, or even absolutely erroneous.

In all other respects we can only endorse the opinions expressed in our former notice of this book. The author has evidently bestowed immense labour on its preparation, and while we admit that there are portions of the work which are fairly open to criticism, it would be ungracious to point out small defects when the general result is so excellent.

THE MOUND-BUILDERS.†

MUCH has been written by American archæologists on the subject of those mysterious earth-mounds which are abundantly scattered through the valleys of the Mississippi and the Ohio. Mr. MacLean has done useful work in concentrating the literature of the mounds so as to bring its essence within compass of a small octavo volume. Those readers who have not the opportunity of procuring, or the time for reading, the larger works on American archæology, will find in this little book a popular summary of what is known about the mounds and their contents. The implements in stone and

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* *Zoological Classification: a Handy Book of Reference, with tables of the Sub-kingdoms, Classes, Orders, &c., of the Animal Kingdom, their Characters, and Lists of the Families and principal Genera.*—By Francis P. Pascoe, F.L.S. Second edition, with additions and a Glossary. Sm. 8vo. London: Van Voorst. 1880.

† *The Mound-Builders, being an account of a remarkable people that once inhabited the Valleys of the Ohio and Mississippi; together with an investigation into the Archæology of Butler County, Ohio.* By J. P. MacLean. 8vo. Cincinnati: Robert Clarke & Co. London: Crosby, Lockwood & Co. 1879.

copper, handmade pottery, the quaintly wrought pipes, the great plates of mica and lumps of galena, brought from a distance and deposited in the mounds, are relics which tell of a primitive people dwelling in the country before the Red Man got possession of the land; but who these mound-builders were, whether Nahoans or Toltecs, or what not, no man at this day can confidently assert. Notwithstanding the multitude of relics which have been unearthed, we may still say of this subject, as old Camden said of quite another matter, 'we walke in a mirke and mistie night of ignorance.' It should be remarked that Mr. MacLean not only gives a general account of the mounds, but includes in his book an original archæological essay on the mounds of Butler County, in Ohio.

ELECTRIC LIGHT.*

THE recent popularity of the Electric Light seems to have produced a large crop of literature on the subject, of very unequal value and character. The present work is stated in the Preface 'to contain a general account of the means adopted in producing Electric Light.' 'No attempt has been made to teach the science of electricity.' To this modest undertaking of Mr. Urquhart, Mr. Webb adds a prefatory note to the effect that 'in revising for the press Mr. Urquhart's little work, he has endeavoured to arrange the matter in accordance with the history of the subject, and to make a few additions on historical, theoretical, and experimental points.' Beyond these statements, it is not very clear in what proportion the two writers named on the title-page have severally contributed to the general result. A short introduction leads to a chapter on voltaic batteries, very trivial in character, at times incorrect, as, for instance, in stating that 'the dense variety of carbon known as graphite, found in gas retorts, is of little use;' and again, that 'it is better to give the carbons in Bunsen batteries a heading of lead,' and to solder a piece of copper to the top of the platinum for a Grove's battery. The author's modification of 'Burnes' negative plate cell' is stated to be 'very portable, and may be made use of in travelling, to secure photographs of caves and such places.' The negative consists of a plate of copper, to one surface of which, as well as to its edges, a sheet of platinum foil, compact and free from pinholes, is soldered, and to the opposite surface, or back, a sheet of lead.' The exciting solution used consists of sulphuric acid and bichromate of potash, substances calculated to do no little havoc on this singular compound of platinum, lead, copper, and soft solder. Air is moreover pumped into the cells by means of leaden pipes, inserted in the liquid! Ten of these batteries are stated 'to produce the effects of sixty or seventy Grove or Bunsen cells!' 'To prevent the possibility of any disappointment in the use of this apparatus, it will be as well to tell the reader at once, that for every fifteen minutes or so of Electric Light, the cells will be nearly exhausted, and to continue at full power, ought to be refilled.'

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* *Electric Light; its Products and Use, &c.* By J. W. Urquhart, C.E. Edited by F. C. Webb, M.I.C.E., M.S.T.E., with ninety-four illustrations. Small 8vo. London: Crosby, Lockwood and Co. 1880.

Chapters written in the same slipshod style follow, on Thermo-electric batteries (5 pages), magneto-electric generators, electro-magneto-electric machines (6 pages), dynamo-electric machines, evidently gathered from familiar sources, containing nothing new except what is not true; and so at the 150th page we arrive at some 'general observations on machines.' Here, at last, we meet with traces of practical, if not of theoretical knowledge, such as recommendations 'to start the machine with open circuit,' and 'never to place the machine at full speed on short circuit,' to drive steadily, and to use sperm oil for lubrication. 'A machine that heats much is not properly constructed, and may be improved by taking a layer of wire off the electro-magnet.' Chapter VIII. deals after the same fashion with 'electric lamps and candles,' naming those best known, and omitting, as usual, their common ancestor, the Chapman lamp, already figured in this journal. The author contributes a lamp about as good as his battery. Of this invention it is naïvely said, 'When the lamp has given light for about six hours or more, and it is required to continue for another six hours, it is only necessary to slightly lower the plates by the screws in the armatures. *In practice this cannot be done just as directed.*'

Mr. Edison is dismissed very cavalierly. 'Much interest,' says our author, 'has been taken in the sensational and often absurd announcements concerning the apparatus in course of perfection by Mr. T. A. Edison, of Menlo Park, New York. This inventor is well known by his talking phonograph (*sic*) and telephones, and it was in some quarters thought that when he had set himself to the task of inventing an efficient subdivision of the electric light circuit, something would in all probability be done. Unfortunately, however, as far as can be learned up to this date (July, 1879), the attempts have proved almost complete failures.'

In Chapter IX. we have nine pages on the measurement of the electric light, in which Rumford's and Bunsen's photometers are very feebly described, followed in Chapter X. by eleven pages on the 'mathematical and experimental (*sic*) treatment of the subject.' Hopkinson, Schwendler, Preece, and Siemens, are laid under contribution, but to very little purpose. The book concludes with about a dozen pages on the cost of the light. The quantity of information given in 290 pages is infinitesimally small.

Messrs. Crosby and Lockwood deserve much credit for the getting up of the book. The print is excellent, the paper beautiful, the woodcuts superb. Their motto on the title-page is *Capio lumen*—it would have been better in this case had they modified the first word to *Rejicio*. W. H. Stone.

BRITISH COPEPODA.*

In the course of the thirty-six years of its existence the Ray Society has certainly well fulfilled the purpose for which it was established. A great number of copiously, and often most beautifully, illustrated monographs

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* *A Monograph of the Free and Semi-parasitic Copepoda of the British Islands.* By G. Stewardson Brady, M.D., F.L.S. 2 vols. 8vo. London: Ray Society. 1878-1880.

have been issued to the subscribers; works which it would have been impossible for any publisher to bring out in the same style without certain loss; and we are glad to take the opportunity of the appearance of the second volume of Mr. Brady's book on the British Free and Semi-parasitic Copepoda to remind our readers of the existence and activity of this Society, which certainly merits a larger share of patronage than would appear to fall to its lot. This is not the place to give a list of its publications, but we may cite the magnificent work of Alder and Hancock on the Nudibranchiate Mollusca, Forbes' Naked-eyed Medusæ, Professor Allman's Freshwater Polyzoa, Mr. Blackwall's British Spiders, and Mr. Darwin's Monograph of the Cirripedes, as showing how much naturalists are already indebted to the Ray Society.

Among the earlier volumes published by the Society was Baird's *Natural History of the British Entomostraca*, an excellent and standard work, founded to so great an extent upon the author's personal investigations that although it is now somewhat out of date it must always hold its position as a book of reference. It was published in 1850; and the best proof of the vast strides that have been made in this department of Zoology, of late years is to be found in the fact that Mr. Brady has now devoted two octavo volumes to the description of the members of only one of the six orders recognized by Baird. The truly parasitic forms, whose alliance with the crustaceans described by Mr. Brady is, in many cases, established only by the investigation of their development, were placed by Baird in a separate division of the Entomostraca; and this distinction is still maintained by some high authorities, so that his Copepoda include precisely the free-swimming and semi-parasitic (or commensal) forms which form the subject of these volumes.

Of his Copepoda, Baird describes and figures only thirteen species, but in the case of the well-known freshwater *Cyclops* he cites three varietal forms, so that we may reckon that he recognized fifteen forms in the order. Mr. Brady, however, describes fourteen species of the genus *Cyclops*, as now restricted, whilst the total number of British Copepods with which he expects to have to deal in the execution of his present task, amounts to at least 151. Of these, 134 are treated of in the volumes before us, the remainder, consisting of three families of somewhat aberrant and semi-parasitic forms, will form a third volume, which will also contain a general sketch of the anatomy and physiology, and development of these organisms. In his Introduction, Mr. Brady gives short directions as to the best modes of collecting, examining, and preserving the Copepod Crustacea.

Of the systematic part of the work, considering the reputation of the author, it is scarcely necessary to say much. As we have already remarked, he differs somewhat from certain other writers in his views as to the affinities of the free and parasitic Copepods, and this, to a certain extent, influences his opinions as to the classification of the former; but only with regard to certain doubtful forms, as to which an absolute agreement between different geologists is scarcely to be hoped for. In every respect the work is most carefully executed, and it cannot fail to prove of the greatest advantage to the students of our British fauna. The plates, of which there are in all eighty-two, furnish outlines, and sometimes coloured figures of the species, with copious illustrations of the structure of those parts which are

of most systematic importance. These figures have been drawn by the author himself, and carefully lithographed by Mr. Hollick. They, of course, add immensely to the value of the work.

THE GEOLOGICAL RECORD.*

WE are exceedingly glad to be able to call attention to the publication of another volume of this valuable guide to contemporary geological literature. The present issue has been, indeed, a little tardy in making its appearance; but the editor informs us in his preface that this is due to the temporary loss of the MS. of the section on European Geology, which, he says, was 'appropriated by some very wise person as an article of great value, and not recovered for some months.' The general arrangement of the contents is the same as in the last volume, but we observe that the editor has adopted an approximation to the plan which we ventured to suggest in noticing the latter, by placing the supplementary notices relating to former years as appendices to the sections to which they belong, instead of bringing them all together in a miscellaneous mass at the end of the book, where they stood in much danger of being overlooked. This is, undoubtedly, a great improvement, but we still think it would be better to do away with supplements altogether, and work their contents into the body of each section, merely distinguishing the supplementary articles by the addition of their dates. Mr. Whitaker does not claim perfection for the work offered to their *confrères* by himself and his able coadjutors; indeed, he insinuates that the attainment of perfection might be unsatisfactory to some people who are fond of 'discoursing eloquently of "the imperfections of the *Geological Record*;"' but we are thankful to say, that the fear of such a catastrophe does not influence these 'Able Editors,' who evidently aim at making their Records of Geological Literature as perfect and useful as possible, and certainly with no stinted measure of success.

NEW ZEALAND ZOOLOGY.†

PROF. HUTTON'S '*Zoological Exercises*' is the outcome of his adoption of the method of teaching Natural History advocated and illustrated by Professors Huxley and Martin in their *Elementary Biology*. His students required a handbook, and that just mentioned, from its dealing only with British organisms, and for some other reasons, did not quite suit the wants of the New Zealand Professor, who, accordingly, set himself the task of pre-

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* *The Geological Record for* 1877. *An account of Works on Geology, Mineralogy, and Palæontology, published during the year.* With Supplements for 1874-76. Edited by William Whitaker, B.A., F.G.S. 8vo. London: Taylor & Francis, 1880.

† *Zoological Exercises for Students in New Zealand.* By F. W. Hutton. Sm. 8vo. Dunedin: J. Wilkie & Co., 1880.

paring a manual which should deal with characteristic types of animals inhabiting New Zealand, more or less after the same fashion.

It will be seen that in one important respect Prof. Hutton departs from the method advocated by Prof. Huxley, namely, that of regarding Zoology and Botany as forming one single science; further, while noticing a much larger number of animal types, his investigation of them is by no means so elaborate as that sketched in the English book; but on the other hand, his little work includes certain elements wanting in the latter, and which will, we think, peculiarly adapt it to be useful to those who are seeking to instruct themselves in isolated and out-of-the-way places. These are a description of the microscope, and a guide to its use, preceding the section on Morphology; and a general synopsis of the classification of animals, and a tabular analysis of the families of insects which follow it. The last-mentioned portion of the contents is intended to enable students to make their first steps in taxonomical zoology. The author wishes his readers to collect numerous insects, and then, by means of this table, to refer them to the families to which they belong.

PONDS AND DITCHES.*

WE welcome this little volume on account of its intrinsic qualities, and also as it may serve to call the attention of a considerable number of readers to the interesting and instructive series of natural history objects that may be obtained from sources that most people pass, either without consideration or with a feeling of contempt. The author has given a good sketch of the botany and zoology of ponds and ditches, indicating, generally, the more striking and interesting members of the various groups of plants and animals that may be found in such localities. So far as it goes his work is a very satisfactory one throughout, and but for two or three objections it might justly have been recommended as a most satisfactory guide to that wealth of organisms that people even the most insignificant pieces of standing water. The author commences by describing the principal flowering plants which grow either in the water of ponds and ditches, or on their margins, then notices, in their order, the lower forms of vegetable existence, the freshwater Algæ, Desmids, and Diatoms, and finally the animals which abound in such places, commencing the latter part of his work with the Protozoa, and working upwards to the insects and their larvæ. But, unfortunately, by what seems to us a great error of judgment, he has omitted all notice of the pond-haunting fishes and Batrachia, which would furnish materials for two interesting chapters, and this because they have already been treated in another volume belonging to the same series. We presume it is for the same reason that the pond Mollusca are also passed over *sub silentio*, and these two omissions seem to us to detract immensely from the value of Dr. Cooke's little volume, which might so easily have been made a very excellent guide to the

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* *Natural History Rambles.—Ponds and Ditches.* By M. C. Cooke, M.A., LL.D. Sm. 8vo. London: Society for Promoting Christian Knowledge, 1880.

mysteries of pond-life. This incompleteness is the more to be regretted as the greater part of the remainder seems to be very carefully prepared, the only chapter that shows striking indications of weakness being that on aquatic insects, in which no mention is made of several forms common in ponds. We may especially notice the curious larvæ of *Stratiomys* and *Eristalis* as being entirely unmentioned, as also the Palpicorn water-beetles, including the great *Hydroïs*, which was certainly worthy of a few words. We notice, further, that the larva of *Acilius sulcatus* is figured as if it was that of *Dyticus;* and that the author's notions of the aquatic Hemiptera are rather confused. The little book is well illustrated with woodcuts. It concludes with a chapter on collecting and examining specimens, and notwithstanding the defects which we have noticed above, it will undoubtedly prove a useful companion to the young naturalist in his first steps towards the knowledge of freshwater organisms.

BRITISH MOSSES.*

MOSSES, perhaps, receive about as little attention from Botanists as any class of plants, and considering how many Botanists are mere collectors, and how admirably mosses lend themselves to the collector's purposes, this is rather remarkable. Something may be due to the minuteness of the size of many of the species, and something perhaps to difficulties inherent in the systematic treatment of these plants; but we fancy the chief cause of the comparative neglect with which they are treated is to be sought in the want of a good illustrated English treatise upon them. The flowering plants and ferns are enthusiastically studied by many; sea-weeds are not wholly neglected; the study of the fungi may almost be regarded as a prevalent fashion, and the lichens have their admirers; but upon all these groups the British Botanist possesses more or less reliable guides in his own language.

In the work of which the first part is now before us, Dr. Braithwaite aims at placing the British Mosses on the same vantage-ground as the more favoured classes of the Vegetable Kingdom; and judging from the sample lately issued, he will succeed in his endeavours. In this first part, indeed, he has not got very far with his task—it relates solely to that curious order of mosses, the Andreæaceæ, which differ from all other members of the class, and resemble the Jungemannieæ, in the longitudinal splitting of their capsules, but show their closer affinity to the true mosses, in the general structure of the plants, the presence of a columella in the fruit, and the absence of elaters. Of these plants, which grow always upon rocks on high mountains, extending up to the borders of the perpetual snow, the author distinguishes five British species, which he describes fully, indicating and discussing their synonymy in detail, and illustrating their structural peculiarities by numerous figures drawn by himself, and forming two plates.

The work, if carried on as it has been commenced, must attain con-

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* *The British Moss-Flora.* By R. Braithwaite, M.D., F.L.S. Part I. 8vo. London: Published by the Author (at 303 Clapham Road). 1880.

siderable dimensions, but it bids fair to furnish British Botanists with a very complete Hand-book of the Mosses, embodying the results of recent foreign work, even to the extent of indicating the extra-British European species. Our only regret is that the author should have adopted the very inconvenient system of personal publication, which always increases the difficulty of procuring a book.

SPRING.*

IT is rather remarkable in these years to find any one capable of getting enthusiastic on the subject of Spring. Of course there are many effete articles of faith to which some men cling tenaciously for years after they have been proved to have no real foundation; but these generally rested originally upon intangible bases; and it is well known that many people (especially women) persist in the belief that certain other persons are angels, whom the general voice of society would pronounce to be of a very different quality. Such delusions must be matters of memory, and to the same category we must refer Mr. Heath's encomiums on Spring. For the last few years, indeed, it has been rather hard to say when spring began or ended. The whole year has been one continuous wet March, varied only by frost and snow and fogs, in those months which have always been denominated winter, and by occasional hot days in the season which is still, by courtesy, named summer; and it can only be by an effort of memory that even a poet could sing the charms of spring. To avoid confusion (influenced probably by the peculiar meteorological conditions above referred to), Mr. Heath makes spring commence in January. He might also, with some show of justice, have made it include June, but, yielding to old prejudices, he contents himself with May.

The earlier chapters or sections of Mr. Heath's *Sylvan Spring* contain general descriptions of the delights of country, and particularly of woodland scenery in the early part of the year, and this part of his work especially we must regard as embodying reminiscences of the past and anticipations of the future rather than the records of present experiences. The sun shines brightly in Mr. Heath's pages; the flowers bloom, the birds sing, and the trees burst into leaf as they used to do in days long past, and the whole story of Spring is told with a most enviable freshness. In the later sections our author ties himself down to a chronological arrangement of his subject, and treats successively of the months from January to May, indicating under each the principal phenomena which may be observed in the progress of vegetation, and interspersing his narrative with some notices of birds and insects, among the latter, especially, the butterflies. The book is illustrated with numerous woodcuts, among which some of those representing country scenes are exceedingly beautiful; and also with a dozen plates printed in colours, which, although not first-rate productions, are pretty and attractive

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* *Sylvan Spring.* By Francis George Heath. Sm. 8vo. London: Sampson Low & Co., 1880.

enough. Like all Mr. Heath's works, this seems to be well adapted to its purpose of exciting a love of nature and a desire for studying natural objects in its readers' minds, and few books could be more appropriately placed in the hands of young people who have the good fortune to be able to pass any portion of the earlier months of the year among the rural scenes which it so successfully depicts.

GEOLOGY OF THE LONDON DISTRICT.*

WE are glad to be able to announce the publication of a third edition of Mr. Whitaker's memoir on the Geology of the London district, the explanation of the Geological Survey map of that area, and of the geological model of London, which may be seen in the Museum of Practical Geology in Jermyn Street. The book, as we stated in noticing its first issue, affords an excellent summary of the geological phenomena observable in London and its immediate neighbourhood, and Mr. Whitaker has carefully worked into it the results of recent investigations, some of which are of considerable interest and importance. The most important of all the additions are those relating to the demonstration of the existence of a great ridge of Palæozoic rocks under the London district, upon which so much light has lately been thrown by deep borings in quest of a supply of water. All these are noticed in some detail, and at p. 19 there is a very useful table showing their depths and the various formations passed through in sinking them.

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* *Guide to the Geology of London and the neighbourhood.* By William Whitaker, B.A., F.G.S. Third edition, 8vo. London: Longmans and Stanford, 1880.

SCIENTIFIC SUMMARY.

ASTRONOMY.

Solar Parallax.—Mr. David Gill, her Majesty's Astronomer at the Cape of Good Hope, has at length completed his investigation of the value of the solar parallax, and he communicates the result to the Royal Astronomical Society. It will be remembered that Mr. Gill went on an expedition to Ascension Island in the year 1877 in order to determine the solar parallax by measuring with a heliometer the distance and position of the planet *Mars* from certain stars in the evening and morning. The expedition proved completely successful. Since this period Mr. Gill has been engaged in reducing these measures with the greatest care, taking every precaution to eliminate every source of systematic error. On twenty-five occasions he was able to obtain a complete series of measures of the distances of the planet *Mars* from the same star in both the evening and morning, this observation being superior to those in which the position of the planet had to be measured from different stars in the evening and morning. These twenty-five sets of measures give for the value of the solar parallax

$$\pi = 8''{\cdot}780 \pm 0''{\cdot}013$$

When all the observations are reduced together, the result is

$$\pi = 8''{\cdot}778 \pm 0''{\cdot}026$$

The larger probable error shows that this result is inferior in accuracy to the other.

The separate results range between 8″·62 and 8″·90, so that they are fairly accordant, and would seem to render it probable that the resulting value,

$$\pi = 8''{\cdot}780,$$

is not so very far from the truth. This value is considerably smaller than is generally believed to be correct, most astronomers considering that the real value is not very far from

$$\pi = 8''{\cdot}84$$

The result of the last transit of *Venus* has given all kinds of value, from 8″·76 to 8″·90, and up to the present time no very satisfactory result has been obtained.

Lately an attempt has been made by Messrs. Campbell and Neison to determine the value of the solar parallax from the parallactic inequality in

the motion of the Moon by taking means to eliminate all the systematic errors which have hitherto rendered discordant the results obtained by this method. The method of determining the solar parallax by means of the parallactic inequality would be, perhaps, the most favourable of all were it found possible to eliminate these systematic errors. This Messrs. Campbell and Neison have undertaken to do. The value they obtain for the solar parallax is

$$\pi = 8''{\cdot}778 \pm 0''{\cdot}007$$

This value is conditional, however, on the assumption that there is a periodical variation in the motion of the Moon due to the action of the planets, which systematically affect the value found for the parallactic inequality. The fuller inquiry whether there really exist such periodical variations, the authors defer to a subsequent research. If there do not exist such variations, then they show that the parallax of the Sun is

$$\pi = 8''{\cdot}848 \pm 0''{\cdot}007$$

Transit of Venus on December 6, 1882.—The Astronomer Royal has lately communicated to the Royal Astronomical Society an account of his proposed plans for observing the transit of *Venus* on December 6, 1882. The method to be used is Delisle's, which consists in observing the exact time of the ingress or egress of the planet from points on the surface of the earth where this ingress or egress is much retarded or accelerated.

For places where the ingress of the planet on the Sun will be much accelerated, it is proposed to use stations in the Cape Colony, from the observatory at Cape Town to D'Urban.

For stations where the ingress of the planet on the Sun will be much retarded, it is proposed to utilize places in the West Indian Islands, and especially Barbadoes, Trinidad, and Jamaica.

For places where the egress of the planet from the Sun will be much accelerated, it is proposed to use the same stations as those for retarded ingress.

For stations where the egress of the planet from the Sun will be much retarded, it is proposed to use places on the eastern coast of Australia, including Melbourne and Sydney, and, if possible, New Zealand.

It is proposed to entirely give up the use of photography, on which so much reliance was placed by Sir G. Airy and his advisers prior to 1874, but which the results of that transit showed to be useless when employed in the manner it was.

It may be remarked that the commencement of the transit will be visible in England, commencing shortly before sunset.

Ultra-Neptunian Planets.—For many years astronomers have speculated on the probability of there being several planets belonging to the solar system and revolving in orbits beyond that of *Neptune*. Such a planet would probably be so faint and would move so slowly that it might easily escape detection for many years. There is, moreover, a peculiar relationship between the aphelion distances of a number of periodical comets and the mean distance of the planet *Jupiter*, which has led astronomers to suppose that these comets have been introduced into the solar system by the attraction of that giant planet. There are other comets whose aphelion

distances are similarly related to the planet *Neptune*, and might be supposed to have been introduced into the solar system by the attraction of that planet. For this reason it has frequently been pointed out that there may be ultra-Neptunian planets with mean distances of about 45 and 75 times that of the Earth, as there seems to be a tendency for periodical comets to have their aphelion distances at about one of these distances from the Sun. Nothing beyond mere speculation seems to have engaged the attention of astronomers. At the end of April, however, Prof. George Forbes published the following definite statement:—'Prof. G. Forbes, of Anderson's University of Glasgow, has reason, from the study of the action of the planets on comets, to believe that a planet exists at present in right ascension $11^h\ 40^m$ and North Polar distance 87°, or within, perhaps, 5° or so of that spot, close to the ecliptic; that the opposition is just past, and that its distance from the Sun is 100 times that of the Earth.'

Much interest was aroused by this statement, and much curiosity excited as to the means by which Prof. Forbes had arrived at so definite a prediction of the position of so distant a planet. It was obvious that he could not have made use of a method of inverse perturbations, like the method employed by Adams and Leverrier for the discovery of the position of *Neptune*, for, apart from other considerations, this would require his proving that there was no other planet between *Neptune* and this new one, which may be termed Forbes' planet, and this is impracticable with our present knowledge.

It was evident, therefore, that he must have employed some method based on the position and dimensions of the orbits of the known periodical comets. It was by no means obvious, however, how from these data there could be determined the present position of the planet, even granting that it might be legitimately assumed that such a planet had introduced certain comets into the solar system.

Lately Prof. Forbes has published some details of his method of investigation. Prof. Forbes assumes, firstly, that any comet with a given aphelion distance has been introduced into the solar system by a planet with a mean distance sensibly equal to the aphelion distance of the comet; and secondly, that this comet was introduced into the solar system at a time when the comet was at its aphelion and when the longitude of the planet was nearly coincident with the longitude of the aphelion of the comet. Both of these are fair assumptions to work on. He shows that there is a group of comets with period ranging between 300 and 500 years, which have their aphelion distance at about 100 times the mean distance of the Earth. In accordance with the basis of his investigation he assumes that these comets have been introduced into the solar system by the attraction of a planet whose mean distance is about 100 times that of the Earth, and whose period of revolution around the Sun is about 1000 years.

Prof. Forbes then inquires what must be the position of this planet at the present time, so that by its motion in past ages this planet would have been in the right position to introduce these comets into the solar system. In a highly ingenious manner Prof. Forbes shows that if the present longitude of the planet be about 180° and it moves about $\frac{1}{3}$° per year, it would have been in past ages in the right position to introduce into the solar system the greater number of the comets in this group. Thus, in about the year 1650

it might have introduced the comet I. of 1843, and in the year 1608 have just moved into the right position to introduce comet II. of 1850. Still earlier, in 968, it would have been in the position to introduce comet IV. of 1840, and in the year 409 the comet I. of 1861. Assuming, then, the planet did introduce these comets at these times, Prof. Forbes is able to show that the planet must have a period of about 1006 years, a mean distance of $100\frac{1}{2}$, and be in longitude 174° in the year 1880. Consequently its present position would be Right Ascension $11^h\ 40^m$, and North Polar Distance 87°. Prof. Forbes then inquires if there be any stars now missing from the Catalogues which may be really observations of this planet. The only one he finds is star No. 894 of the *Greenwich First Seven-Year Catalogue*. This is a star which was observed twice in the year 1857, but on no subsequent occasion, and its magnitude is not stated, but cannot have been less than eighth or ninth magnitude. It is not unlikely that this supposed star may have had no existence, and the observation have been that of some known star, written down with a wrong reading, or it may have been a faint star observed by mistake, having been taken for a minor planet, which would account for its not being observed again; but, as Prof. Forbes points out, it may have been his planet, which would have been quite close to the place at the time.

If this planet really exists, what would be its probable dimensions and appearance? *Uranus* and *Neptune* are both about 35,000 miles in diameter, and it may be assumed that the new planet is of the same dimension. Then, from its great distance, it would present a disc only 0″·8 in diameter, or practically undistinguishable in size from that of a star, unless it be with a very large and perfect instrument. Its brightness would also be much less than that of *Neptune*, as not only would it receive but one tenth of the amount of light, but there would be only one-tenth as much reflected to the Earth, so that it would shine with only one-hundredth of the brightness. The brightness of *Neptune* is that of a star of the eighth magnitude, so that the brightness of the new planet would be only that of a star of the fourteenth magnitude. Its mean daily heliocentric motion would be $3\frac{1}{3}''$ of arc, which would be so slow that it could only be detected after several days' interval. Considering, therefore, the very great number of fourteenth-magnitude stars which there are in any small area in this portion of the heavens, probably many hundreds in the area which must be searched to find this planet, and the necessity that there would be of mapping the whole district as practically the only way of detecting so slow a motion, it may be regarded as almost hopeless to search for such a body. It is probable that the planet would not even be visible with less aperture than 9 or 10 inches, and it would require a telescope of at least 15 or 16 inches aperture to properly search for it. Unless, therefore, this new planet of Forbes is much larger than either *Neptune* or *Uranus*, its discovery is well-nigh hopeless.

But if the missing star from the *Greenwich First Seven-Year Catalogue* be really this new planet, it must shine like a star of the eighth or ninth magnitude. To do this it must have a diameter at least eight times that of *Neptune* or *Uranus*. This would be excessively improbable, for it would imply a diameter of 280,000 miles, or three times that of *Jupiter* and nearly one third that of the Sun, whilst its mass would probably be nearly twenty times that of *Jupiter* or one-fiftieth of that of the Sun, assuming

the mean density of the planet to be less than that of any other. Its apparent diameter would be over 6″, so that it would appear like an exceedingly bright planetary nebula, and could not well be mistaken for a star by the Greenwich observers. Moreover, it would certainly produce well-marked perturbations in the motion of the greater planets. This consideration seems to render it out of the question that the missing star from the Greenwich Catalogue can be Forbes' planet. For it to be really rendered possible for this star to be Forbes' planet, the planet must be supposed to shine very brightly by its own light, and to be of nearly the dimensions of the planet *Jupiter*. It would then shine like a ninth-magnitude star, and might be detected without very great difficulty. The assumption, however, that a planet of this size could be shining brightly by its own light, is one which is far from being probable. Even under the most favourable circumstances it is not likely that an Ultra-Neptunian planet would possess greater dimensions than the planet *Jupiter*. This would render its apparent diameter about equal to that of *Neptune*, and its brightness to that of a star of the eleventh magnitude. The planet's motion being so slow, it would not be an easy object to detect, but it is not improbable that a long search with a good instrument over an area of 5° by 2° would enable such a body to be detected if it existed.

From the above consideration it would appear, therefore, that even if Prof. Forbes' planet really does exist, its detection cannot be an easy matter.

On a Suspected Variation in the Position of the Earth's Axis.—It has been pointed out, that if from any cause the axis of rotation of the Earth does not coincide with the axis of figure, the former will revolve round the latter in a period of about 306 days. Accordingly there ought to appear a small variation in the apparent latitude of Greenwich, deduced from observation of the Pole-star, which would go through all its variations in about ten months. Professor Clerk Maxwell endeavoured to ascertain whether the *Greenwich Observations* for the years 1851–54 gave any indication of the existence of such a variation, but with doubtful results. The subject has been lately taken up by Mr. A. Downing, of the Royal Observatory, who has discussed the observations of *Polaris* made between 1868 and 1877. He finds evidence of a small periodical variation with a ten months' period. The co-efficient of this term amounts, however, to only 0″·075 ± 0″·15.

This would indicate that the axis of figure of the Earth revolved round the axis of rotation in a circle, about six feet in radius, once in ten months. This result is in close accordance with one derived by Professor Peters from a discussion of the observations made at Pulkowa. On the other hand, Dr. Nyrén has arrived at values for this variation in latitude at these different periods, which are discordant with each other, and would indicate some different source as that of the variation.

Winnecke's Comet.—In the *Astronomischen Nachrichten*, No. 2014, there is an important note by Prof. von Oppolzer giving some of the results of his recent researches on the motion of Winnecke's Comet. This is a well-known comet, of short period, and has been carefully observed during its appearances in 1858, 1867, and 1875. From his investigation of the perturbations in the motion of the comet, Prof. von Oppolzer shows that the observations made at these

three appearances cannot be brought into agreement unless it be assumed, (1) that the mass of the planet *Jupiter* must be reduced from Bessel's value $1 \div 1048$ to $1 \div 1051$; or (2) that the comet suffers a similar retardation in its motion to that which Prof. Encke pointed out was indicated by the motion of Encke's comet. The former of these two alternatives seems out of the question, as all recent investigation of the mass of *Jupiter* from the motion of its satellites, as well as Prof. Axel Möller's researches on the motion of Faye's Comet, and Prof. Kruger's investigation of the perturbation of the minor planet *Themis*, agree in confirming Bessel's value for the mass of *Jupiter*. It would appear, therefore, that Winnecke's Comet really experiences a similar retardation of its motion to that which seems to exist in the case of Encke's Comet; this retardation, as it is well known, by diminishing the dimensions of the orbit shortens its period of revolution. Accordingly Prof. von Oppolzer finds, after each appearance, a mean daily acceleration in its sidereal motion of $0''{\cdot}01439$. This, as Prof. Oppolzer shows, corresponds to a value of the retardation in close agreement with that found by Encke from the motion of the famous comet bearing his name. Of late years the existence of this resisting medium has been considered doubtful, for Encke's Comet seems to be the only one showing any well-defined trace of its effects. Prof. Axel Möller had most carefully investigated the motion of Faye's Comet, another of short period, but failed to find any trace of such retardation. Prof. Oppolzer is of opinion that this is not surprising, as the effect of such a resisting medium on Faye's Comet would be so small that its effects could not be disentangled from the effects of the unknown errors in the perturbations.

The question is one of very great interest, and it is very desirable that as much light as possible should be thrown on the subject. It requires a rigid and exhaustive investigation of the motion of all the comets of short period—a work of very great labour. It is probably only in this manner that further light can be thrown on the subject.

The Great Southern Comet, 1880.—From the further particulars which have now reached England, this comet seems to have been a bright comet of considerable size, and if visible, under more favourable conditions, would have been probably of great brilliancy. It moved in an orbit which approached very close to the Sun, so that the comet swept round its perihelion passage with enormous velocity, and rapidly rushed off into space. It was not seen until several days after perihelion passage, when its tail was observed as a bright band rising above the western horizon. The comet was moving tail foremost, and the nucleus could not be seen; and even the tail, which was of great length, was only visible in bright twilight. The comet grew rapidly fainter, and as it receded from the Sun the nucleus could be detected as a small bright mass. Long before the end of the month, however, the comet had become so faint that it could no longer be observed, even with the great Melbourne reflector. It is probable, that under more favourable circumstances, this comet would have taken rank as one of the largest and most brilliant comets of the century. It never rose sufficiently above the horizon for even its tail to be visible in England.

Immediately on the arrival of the observations in Europe, the calculation of the orbit of the comet was undertaken by astronomers, and especially by

Mr. Hind and Prof. Weiss. It was found to present a very great resemblance to the orbit of the Great Comet of 1843: a resemblance so close, that it seems almost certain that they must belong to the same body, that is, the comets of 1843 and 1880 must be different apparitions of the same body. This would give it a period of 37 years.

The great comet of 1843 was one of the most magnificent comets which have appeared. It suddenly made its appearance on February 28, having passed its perihelion passage on the previous day, and was so brilliant that it was distinctly visible at midday, quite close to the Sun, and could even be seen in broad daylight with the naked eye. It passed so close to the Sun, that in one day it swept over three-fourths of its entire orbit, and moved off into space with enormous velocity, carrying a great and brilliant tail as it went. This comet excited much attention at the time, and numerous attempts were made to calculate its orbit, and determine whether it had been previously seen. The resulting orbit was uncertain, especially as to the period of the comet, the observations being too close together to enable its true period to be made out. Any period from 7 to 700 years seemed possible.

It was suspected that this comet of 1843 might be identical with a very similar comet which suddenly made its appearance in March 1668, and was remarkable for its great and brilliant tail: its orbit seems to be almost identical with that of 1843. Another comet which it was also thought might be identical with that of 1843, was the comet seen in 1689; the orbits closely resembling each other. So do the orbits of brilliant comets which became suddenly visible in 1695 and 1702. The very bright comet of 1618 also seems to have had a similar orbit, and to have been of very analogous character. For this reason it was suggested that perhaps all these comets were apparitions of the same body, with a period of seven years, though it seemed extraordinary that it should not have been visible between 1702 and 1843.

If the period be taken at thirty-seven years, as indicated by the interval between 1843 and 1880, it is obvious that its prior appearances must have been in 1732, 1695, 1658, and 1621, which would exclude all these comets but that of 1695, which both Mr. Hind and Prof. Weiss think was probably an apparition of this comet. It is also thought that it might possibly be the same comet as that seen in 1618. From the position occupied by this comet it is one which may easily be overlooked at any of its reappearances, and, as pointed out by Mr. Marth, may have a period of only eighteen and a half years, as it would pass its perihelion in the intermediate returns in July, and very easily escape detection.

As there is a possibility of the comet having as short a period as seven years, it is probable that astronomers will be on the look-out for it at the epoch of its next possible appearance (January 1887) with the view of detecting it. If properly looked for it might possibly be seen before its perihelion passage.

BOTANY.

Sensitiveness in the Acacia.—Mr. T. L. Phipson has communicated to the French Academy of Sciences (*Compt. Rend.*, 24th May, 1880), some curious

results of experiments made on the leaves of the common acacia (*Robinia pseudacacia*). He finds that when the leaves of this tree are well expanded in bright sunlight, the application of from ten to twenty smart blows with the finger upon the terminal leaflet will cause all the leaflets of the leaf to fold up and go to sleep, just as they do at night. In one case the leaflets occupied five minutes in closing; in another instance four minutes and a half. The leaflets close one after the other, commencing with those nearest the terminal leaflet. Mr. Phipson observed that the leaves required from two to three hours of exposure to the sun before they were again fully expanded.

CHEMISTRY.

Action of Phosphonium Iodide on Carbon Disulphide.—It has been noticed by Hans Jahn (*Ber. Chem. Gesells.*, 1880, xiii., 127), that if phosphonium iodide and carbon disulphide be heated together in sealed glass tubes to 120° or 140° C., the latter is converted, by the hydrogen liberated by the breaking-up of the iodide, into marsh-gas and sulphuretted hydrogen, in accordance with the equation:—

$$C S_2 + H_8 = C H_4 + 2 H_2 S.$$

At the same time, however, a crystallizable body makes its appearance in the form of red needles, which is, probably, a molecular compound of the disulphide and the iodide; by treatment with water it is converted into an amorphous, white, easily-changeable compound, which, according to the result of analysis, appears to have the composition $C_5S_2\ P_6\ H_6\ O_{12}$. By the action of water on the pure phosphonium iodide in presence of carbon disulphide no such compound is found. Its formation from the two bodies may, however, be represented as taking place in the following manner:—

$$5\ C S_2 + 6\ P I_2 + 12\ H_2O = C_5\ S_7\ P_6\ H_6\ O_{12} + 12\ H I + 3\ H_2 S.$$

When boiled with water in closed tubes, the compound decomposes into carbonic acid, sulphuretted hydrogen, phosphorous acid and amorphous phosphorus, in accordance with the equation:—

$$C_5 H_7 P_6 H_6 O_{12} + 6\ H_2 O = 5\ CO_2 + 7\ H_2 S + 4\ H P O_2 + 2\ P.$$

As in the above compound, the atomic ratio between phosphorus and iodine in respect to oxygen is as 1 : 2, the assumption may be held that at the temperature above referred to (120° – 140° C.) the decomposition of phosphonium iodide takes place in accordance with the equation:—

$$2\ H_4 P I = P I_2 + H_3 P + H_5.$$

The new Elements found in Gadolinite and Samarskite.—Since the publication of the paper on Terbium by Marignac and Delafontaine, in March 1878, announcements have appeared respecting no less than ten new earths, mosandrin, philippin, ytterbin, decipin, scandin, holmin, thulin, samarin, and two others without names. As was to be expected, so extraordinary an increase in the number of a series of bodies which are exceedingly difficult to separate and hard to characterize has not been allowed to pass without question. Delafontaine (*Compt. Rend.* 1880, xc. 221) in his investigation on the composi-

tion of gadolinite and samarskite, has always laid great stress on the atomic weights. As soon as a substance shows an atomic weight higher than that of the other members of the same group (for example, decipin and ytterbin) he regards the existence of the new element as established. When, however, its atomic weight comes between those of other members of the same series (for example, philippin compared with yttria and terbia), experiments were continued until the chemist was convinced that the body under investigation was not a mere case of a mixture of others. In such cases a study of the physical characters was of value. On these grounds the author regards as established the existence of ytterbium, decipium, and philippium. Of scandium, he can say nothing, as he has not met with it. Of mosandrium, he is firmly of opinion that it must be struck out of the list of elements. On samarium, Lecoq de Boisbaudran published a note, in August, 1879, and in the preceding February, one on another new earth in samarskite. The proportions of the latter appear to differ but slightly from those of decipin, or a mixture of that oxide with terbia; and the character of samarium, chiefly known by its absorption-bands, leaves the existence of that substance doubtful. Marignac and Soret have shown erbin to be a mixture of several bodies; and this result Cleve, with the help of Thalén, has confirmed. Cleve goes even further in giving names to two metals which he distinguishes by their spectroscopic differences. The extreme red line noticed by Soret, he refers to thulium, and the red and green lines ($\lambda = 640$ and 536) he terms holmium. Samarskite contains but little of the old erbia; the spectrum of its solution shows it but faintly, especially the green and red absorption-bands ($\lambda = 523$ and 488); the indigo line, on the other band ($\lambda = 462$), is stronger. After removing the didymium, decipium, and terbium compounds, Delafontaine submitted their formiates to fractional recrystallization: the first products gave 49-47 per cent of base; they were purified and again recrystallized, whereby a product rich in terbium was obtained, and in the mother liquors another rich in yttrium. The residue, rendered incandescent, and then converted into nitrates, showed the indigo band 452 strongly; the others weaker. This nitrate, whether dissolved or in crystals, was completely colourless, and had an equivalent = 98. By fractional decomposition, and by ignition, it was separated into six products (A to F): A had an equivalent = 102, the formiate had a light rose colour, and the absorptive spectrum showed the line of impure erbia very strongly. B to E was more strongly coloured, the nitrate colourless, the spectrum showed only the band in the red 640, in the green 536, was small; and in the indigo, 448–445, broader; the line of pure erbia was reduced to a minimum. The earths, in B to E, cannot be separated into terbia and yttria in the usual way. The product F, the last we have to consider, gives a very weak spectrum; and its colour, as well as its equivalent, was less than that of the substance in the original material. The author has given the name philippin to the yellow oxide differing from terbia, which forms the chief constituent of the oxide above referred to. Its properties accord with those of the X earth of Soret, and the holmin of Cleve, so that the latter name should fall into disuse. Philippin may be regarded as consisting of two oxides, one giving the indigo line 448–445, and the other the band 640 and 536; the author, however, has met with no facts which favour this view. The so-called euxenite of North Carolina contains even less of earths

with absorptive spectra than samarskite. In addition to terbia there was found a yellow earth with an equivalent of about 90, and without an absorptive spectrum, which appeared to be a mixture of terbia and yttria.

The Vapour Density of the Halogens.—The researches on this subject, carried on by Victor Meyer, in association with Carl Meyer, on the dissociation of chlorine at a red heat, have opened the field to a great number of researches, the publication of which they withhold till all the difficulties incident to the inquiry have been overcome. In the meantime, Crofts has published in the *Comptes Rendus* the results of an investigation carried out according to the method proposed by Meyer, and they agree with those of Meyer which had not yet been made known. This has led Victor Meyer to publish his results in a series of short notices in the *Rev. deut. Chem. Gesellsch.* (1880, xiii. 34). They are directed, in the first place, to the comportment of the two other halogens at a red heat, then to the conditions under which the dissociation occurs and is persistent, and finally to the cause of it. In his paper on chlorine Meyer pointed out that iodine comported itself similarly. At 600° the density of iodine vapour corresponded exactly with the formula I_2; at 800° a considerable diminution of the density was observed, and from 1027° to 1567°, through a range of temperature extending over 500°, it was fixed and unalterable corresponding to $\frac{2}{3}$ I_2. Iodine differs from chlorine in that the decrease of density, amounting to $\frac{1}{3}$, is reached at a lower temperature: chlorine only at 1200° is completely reduced to $\frac{2}{3}$ Cl_2, while iodine shows the complete change at about 1000°. Bromine showed the same change at 1570°, when the density fell to 3·78 and 3·64; the theoretical density of Br_2 is 5·52, and of $\frac{2}{3}Br_2$ 3·64. It therefore follows the same law, its density diminishing to the extent of $\frac{1}{3}$rd its normal value at a yellowish red heat. The dissociation of chlorine to molecules of the size $\frac{2}{3}$ Cl_2, which when nascent chlorine from platinum protochloride is employed occurs at a yellow red heat, does not take place at the same temperature when ready-prepared chlorine is employed in its place; iodine, on the other hand, undergoes the change in either case. The capability of Cl_2 and $\frac{2}{3}$ Cl_2 of existing at the same temperature, which the foregoing statement implies, is analogous to that of oxygen and ozone, which, as regards their dissociation, comport themselves in exactly the same manner. It is not a little curious, that while Deville and Troost noticed no change in the density of iodine vapour at 1000°, both Meyer and Crofts have observed the diminution of $\frac{1}{3}$rd its density at that temperature: the former places the iodine in a cold vessel and slowly heats it; the latter places it at once in the highly heated apparatus.

GEOLOGY AND PALÆONTOLOGY.

Pre-Cambrian Rocks in the Scottish Highlands.—According to Dr. Hicks, in a paper communicated to the Geological Society, the North Western and Central Highlands of Scotland include the following districts in which the rocks are wholly or in part pre-Cambrian:—

(1) *Glen Finnan, Loch Shiel to Caledonian Canal.*—In the former district

the rocks are gneiss, often massive. In Glen Firmilee is a series which the author regards as newer and Pebidian. At Farofern are quartz rocks which the author identifies with those beneath the limestone in Glen Laggan, near Loch Maree, and probably of Silurian age. At Bannavie is a granite which the author considers to be Pre-Cambrian.

(2) *Fort William and Glen Nevis.*—In this district chloritic schists and gneiss occur, which the author regards as Pebidian.

(3) *Ballachulish, Glen Coe, and Black Mount.*—Chloritic schists and quartzites occur here, followed near Loch Leven unconformably by Silurian rocks. On the east of the Ardsheal peninsula, there is granite, which the author believes to be Pre-Cambrian. Going eastward from Ballachulish we have slates, probably of Silurian age. In Glencoe are granite-banded felsite, gneiss, breccia, resembling as a whole the rocks of the Welsh Arvonian group. Between the Black Mount and Loch Sullich are traces of a great Pre-Cambrian axis, bringing up the gneissic series; this is traceable also towards Glen Spean and Loch Laggan to the N.E.

(4) *Tyndrum to Callander.*—South and east of the former are gneisses and silvery mica-schists. Crystalline limestones and serpentines are associated near Loch Tay, resembling those in the Pebidian series of North Wales.

Dr. Hicks states that the Silurian (and Cambrian) rocks flank the Pre-Cambrian in lines from N.E. to S.W., and overlap Ben Ledi on the south side. Thus here, as elsewhere, subsequent denudation has removed enormous masses of the more recent rocks, only here and there leaving patches of these in folds along depressions in the old Pre-Cambrian floor.

Fossil Glutton in Britain.—Mr. E. T. Newton has announced, in the *Geological Magazine* for April, and in a paper read before the Geological Society, the occurrence in the forest bed of Mundesley, Norfolk, of a portion of the lower jaw of a glutton (*Gulo luscus*), a carnivorous mammal now confined to high northern latitudes. The specimen shows the first true molar, and the hinder half of the fourth pre-molar in place, and thus furnishes all requisite characters for its identification. It is rather smaller than the same part in the recent glutton. This is the first discovery of the animal in a fossil state, except in caves.

Fossil Chelonia.—Professor Seeley has announced to the Geological Society that after a careful examination of the specimen which Von Meyer described as probably representing an Edentate mammal, allied to *Glyptodon*, under the name of *Psephophorus polygonus*, he found that, as surmised by Professor Fuchs, it was really a Chelonian allied to the Leathery Turtle (*Sphargis*). The dermal skeleton is made up of irregularly polygonal plates of various sizes, closely resembling those of *Sphargis*, except that each plate is almost twice as large as those of that form. The plates usually show a radiate ornament on the surface. On the underside of the slab are the remains of several vertebræ, apparently from the base of the neck, and these differ from the vertebræ of all known Chelonians in having strong transverse processes for the attachment of ribs. The neural arch, like the processes, is anchylosed to the centrum. The author considers that the dermal skeleton is not represented in the carapace of ordinary Chelonia, but is represented by the granulations on the surface of the carapace of the Trionychidæ. He is hence

led to indicate three primary divisions of the Chelonian order—viz. 1. *Aspidochelyidæ*, in which the bony carapace is covered with symmetrical horny scutes, including Turtles, Emydians and Tortoises; 2. *Peltochelyidæ*, in which the bony carapace has a granular surface-structure, and is covered with an undivided dermis without scutes, including only the Trionychidæ; and 3. the *Dermatochelyidæ*, in which the carapace is not developed, but is functionally represented by a bony skeleton within the skin, as in *Sphargis* and *Psephophorus*.

Bohemian Geology.—Mr. J. E. Marr, in a paper read before the Geological Society, has given the results of his investigations of the older Palæozoic rocks of Bohemia, and they are of especial interest, especially in regard to the peculiar theory of colonies put forward by the great Bohemian geologist, M. Barrande. Mr. Marr commenced with a brief notice of the Pre-Cambrian rocks, which are gneisses and schistose limestones with intrusive eclogite; over these lie unconformably green grits, ashes, breccias, hornstones (étage A of Barrande), which the author considers to represent the Harlech Group of Wales. Etage B is unconformable with this, but conformable with C, which contains the 'primordial' fauna of Barrande; D contains the colonies. E to H are Silurian, and more calcareous than those underlying them. The base of the group is unconformable with those beneath. The following are the associated igneous rocks:—Granite, Quartz-felsite, Porphyrite, Mica-trap, Diabase, Diorite, Eclogite. The author made a comparison of the various strata with English deposits. The Pre-cambrian series much resemble the Diametian and Pebidian of Wales, the latter being étage A; étage B, the Harlech; étage C, the Menevian, probably a deep-water deposit, as is indicated by the abnormal size of the eyes of its Trilobites; the lowest bed of étage D probably represents part of the Lingula Flags of Britain. D a, 1, β seems to represent the Tremadoc Shale of Britain, and, like it, contains pisolitic iron-ore. Representatives also of the Arenig and Bala beds are found. A slight unconformity marks the base or the Silurian. Three Graptolitic zones occur. The lowest, or *Diplograptus* zone, identical with the Birkhill Shales, contains thirteen species of Graptolites; the next, or *Priodon* zone (four species), resembles the Brathay Flags; the upper, or *Colonies* zone (five species), resembling the Upper Coldwell Beds of the Lake-district. Above these follow representatives of Wenlock, Ludlow, and probably of the Passage beds. The author, with the evidence of these, discussed the 'colonies' theory of M. Barrande, pointing to the non-intermixture of species, notwithstanding the irregular repetition of the zones, the non-occurrence of these colony-species in intermediate beds, and other reasons. The stratigraphy and palæontology of several of these colonies was discussed in detail, showing it to be probable that their apparent intercalation with later faunas is due to repetition by faulting.

MINERALOGY.

The Emmet County Meteorite.—This curious meteorite fell near Estherville, Emmet County, Iowa, lat. 43° 30′ N., long. 94° 50′ W., within that region of the United States which has been remarkable for falls of meteorites, three

having fallen at Rochester in Indiana, Cynthiana in Kentucky, and Warrington in Missouri, within the space of a month. The phenomena attending the fall were of the usual character, but on a grander scale. It occurred about five in the afternoon on May 10th, 1879, with the sun shining brightly. In some places the meteorite was plainly visible in its passage through the air, and looked like a ball of fire with a long train of vapour or cloud of fire behind it; and one observer saw it a hundred miles from where it fell. Its course was from north-west to south-east. The sounds produced in its course are described as being 'terrible' and 'indescribable,' at first louder than the largest artillery, followed by a rumbling noise as of a train of cars crossing a bridge. Two persons were within two or three hundred yards of the spots where the two larger masses struck the earth. There were distinctly two explosions: the first took place at a considerable height in the atmosphere, and several fragments were projected to different points over an area of four square miles, the largest going furthest to the east. Another explosion occurred just before reaching the ground, and this accounts for the small fragments found near the largest mass. The largest mass fell within two hundred feet of a dwelling-house, at a spot where there was a hole, six feet deep, filled with water. The clay at the bottom of the hole was excavated to a depth of eight feet before the meteorite was reached. Two or three days elapsed before it was reached. The second large mass penetrated blue clay to a depth of five feet, at a spot about two miles distant from the first. The third of the larger masses was found on the 23rd February of the present year at a place four miles from the first, in a dried-up slough. On digging a hole the stone was met with at a depth of five feet. The fragments thus far obtained weigh respectively 437, 170, 92½, 28. 10½, 4, and 2 pounds. The height of the meteor is calculated to have been forty miles, and its velocity from two to four miles per second, The masses are rough and knotted, like large mulberry calculi, with rounded protuberances projecting from the surface on every side. The black coating is not uniform, being most marked between the projections. These projections have sometimes a bright metallic surface, showing them to consist of nodules of iron; and they also contain lumps of an olive-green mineral, having a distinct and easy cleavage. The greater part of the stony material is of a grey colour, with the green mineral irregularly disseminated through it. The masses vary very much in density in their different parts, the average cannot be less than 4·5. When a mass is broken, one is immediately struck with the large *nodules* of metal among the grey and green stony substance; some of them will weigh 100 grammes or more. In this respect this meteorite is unique; it differs entirely from the siderolites of Pallas, Atacama, &c., or the known meteoric stones rich in iron, for in none of them has the iron this nodular character. The larger nodules of iron appear to have shrunk away from the matrix; an elongated fissure of from two to three millimetres sometimes intervenes, separating the matrix and nodules to the extent of one-half the circumference of the latter. The only mineral which could be picked out separately has an olive-green colour; it occurs in masses, from one half-inch to one inch in size, has an easy cleavage in one direction and was found to be olivine. The same mineral occurs in minute rounded concretions in other parts of the material; and minute, almost colourless crystal-

line particles in the centre are also supposed to be olivine. Troilite exists in small quantity. A quantity of the silicates was picked over, separated as far as possible from iron, and treated with hydrochloric acid. The ratio of soluble to insoluble silicate varies very much in different parts of the meteorite, varying from 16 to 60 per cent for the soluble part. The insoluble part consisted of

		Oxygen
Silicic acid	54·12	29·12
Iron protoxide	21·05	4·67
Chromium oxide	trace.	
Magnesia	24·50	9·80
Soda with traces of K and Li	·09	·023
Alumina	·03	·013
	99·29	

This is evidently the bronzite commonly found in meteorites.

The green mineral is the soluble part of the meteorite; its cleavage in one direction is very perfect, its specific gravity is 3·35, it has a hardness of almost 7, and is readily and completely decomposed by hydrochloric acid. On analysis it was found to have the composition:

		Oxygen
Silicic acid	41·50	22·13
Iron protoxide	14·21	3·12
Magnesia	44·64	17·86
	100·35	

This mineral, therefore, is olivine. Dr. Lawrence Smith, who has examined this meteorite, describes a third silicate which is opalescent and of a light greenish-yellow colour, and cleaves readily. It was a difficult matter to obtain enough of this silicate for analysis, but an examination of 100 milligrammes gave the following numbers:

Silicic acid	49·60	26·12
Iron protoxide	15·78	3·50
Magnesia	33·01	13·21
	98·39	

This is equivalent to one atom of bronzite and one atom of olivine, which, he says, is 'A form of silicate that we might expect to find in meteorites.' The nickel iron, as has already been stated, is abundant, sometimes in large nodules of from 50 to 100 grammes. It displays the Widmanstättian figures beautifully, and possesses the following composition:

Iron	92·001
Nickel	7·100
Cobalt	0·690
Copper	Minute quantity
Phosphorus	0·112
	99·903

A careful examination for felspar and schreibersite was made, but with a negative result. (*American Journal of Science*, June, 1880. [3] XIX. 459.)

On the Artificial Formation of the Diamond.—The second paper giving in detail the method employed with success by Mr. J. B. Hannay in forming diamonds artificially, has recently been communicated to the Royal Society, and is deserving of a short notice. The amount of hydrogen taken up by sodium was determined, and it was found that as much as thirty-two times its volume could be removed from it after it had been allowed to act on paraffin spirit. The sodium was often found to contain carbon of a hard, scaly nature, and 'this was the reaction on which my work was built.' Potassium yielded less satisfactory results; lithium acted better, the carbon which it liberates by the action of paraffin spirit will often scratch glass easily. For the diamond experiments the material was heated in iron tubes, and tube after tube exploded and all was lost. It was often found on boring open a tube that the interior was harder than the exterior and converted into steel. To meet this loss of carbon some lampblack was added to the contents of the tube. Iron tubes $20'' \times 2'' \times \frac{1}{2}''$ bore were used, and three grammes of sodium were employed, the tubes being filled from $\frac{3}{8}$ to $\frac{3}{4}$ full of paraffin spirit and then welded together. The tubes exploded during the heating, and recourse was had to tubes on the coil principle, and two were constructed of the toughest bar iron, made solid, and bored out afterwards. The dimensions were $20'' \times 2\frac{3}{4}'' \times \frac{1}{2}$ bore; the tubes contained three grammes of sodium, $\frac{1}{2}$ gramme of lampblack, and were filled two-thirds full of paraffin spirit. The heat was kept up for eight hours; both kept tight, and furnished a little scaly carbon; other tubes containing lithium burst, and it became evident that still stronger tubes would have to be used. A tube measuring $2\frac{3}{4}'' \times 20'' \times \frac{1}{2}''$ bore was employed, and in it was placed some 'bone oil' (the nitrogenous distillate obtained in the manufacture of bone char), and charcoal powder, and the tube welded up solid. It was heated to a dull red-heat for fourteen hours and allowed to cool. On opening the tube there was a great out-rush of gas, and the carbon was to a certain extent dissolved and some minute portions of it were very hard. It appeared that bone oil had the power of hardening the carbon, and if it acted upon nascent carbon it might harden it so much as to produce diamond. An experiment was made in which bone oil and paraffin were mixed, so that when an alkaline metal was made to act upon it the decomposition of the hydrocarbon might yield carbon which could be crystallized by the action of the nitrogenous liquid. The proportions taken were 90 per cent of bone oil and 10 per cent of paraffin, with lithium as the metal. A very strong tube was filled and welded together and heated to a dull red heat for fourteen hours. When bored open a very high pressure was found inside the tube; the carbon was very hard but could be crushed by agate and would not scratch it. The results, however, when two liquids were used, were so much more satisfactory, that further tubes were filled, welded and heated. A tube measuring $20'' \times 4'' \times \frac{1}{2}''$ bore was filled with four grammes of lithium, bone oil 90 per cent and paraffin 10 per cent, and heated for fourteen hours. When opened, a large amount of gas was given off. In the end of the tube which had been in the upper part of the furnace there was a hard, smooth

mass, adhering to the sides. The tube was cut through at this point and the black mass was pulverized, when some parts were noticed to be extremely hard. On closer examination these parts were found to be mostly transparent, and on triturating them, they were obtained free from the black matter. They turned out to be crystalline carbon, exactly like diamond. From over eighty experiments made with these tubes, Mr. Hannay obtained only three results of a successful nature. The diamond fragments were burnt in a current of oxygen and found to contain 97·85 per cent of carbon. It appeared in some further experiments as if the diamond carbon contained a little nitrogen chemically combined with it.

Artificial Formation of Scorodite.—A concentrated solution of arsenic acid was heated with iron wire in a closed tube to 140°–150°C. The wire was found, by Verneuil and Bourgeois, to be covered in a few hours with a gray, apparently amorphous, material, which soon filled the whole of the fluid. This substance is a mixture of amorphous iron arseniate, and arsenious acid in small crystals. When the heating is continued, this material disappears gradually, and is changed slowly into scorodite; while fresh, apparently amorphous, material is formed, and this is continued till the solution of arsenic acid becomes too weak to continue the reaction. This goes on for about eight days. The crystals, in composition, specific gravity, density, and crystalline form, agree in every respect with the natural crystals of scorodite. (*Compt. Rend.* 1880, xc. 223.)

PHYSICS.

Silver Films in the Camera lucida form the subject of a suggestion by Mr. J. C. Douglas to the Asiatic Society of Bengal. He points out that instruments of this kind are divisible into two classes, the opaque and the transparent. To the former belong Wollaston's and Sœmmering's; to the latter the tinted glass reflector. The former are fatiguing, the latter liable to indistinctness, from double reflection at the two surfaces of the glass plate. Silver films, on the other hand, are so highly reflective that two or more successive reflections may be used. The thickness of the film may, moreover, be modified according to the ratio desired between reflected and transmitted light, and the films may be applied on curved as well as on plane surfaces They may also be found useful in constructing microscopic illuminators.

A New Action of the Magnet on Electric Currents forms the subject of a communication to the *American Journal of Mathematics*, by E. H. Hall. It was proposed to show, that if the current of electricity in a fixed conductor is attracted by a magnet, the current should be drawn to one side of the wire, and, therefore, the resistance experienced should be increased. To test this theory, a flat spiral of German-silver wire was enclosed between two thin discs of hard rubber, and the whole placed between the poles of an electro-magnet in such a position that the lines of magnetic force should pass through the spiral at right angles to the current. The wire was about ½ millim. in diameter, its resistance about 2 ohms. The magnet was worked by a battery of twenty Bunsen cells, joined four in series and five abreast. The strength of the magnetic field was probably 15,000 or 20,000

times H, the horizontal intensity of the Earth's magnetism. Making the spiral one arm of a Wheatstone's bridge, and using a low resistance Thomson's galvanometer, so adjusted as to betray a change of one-millionth in the resistance of the spiral, Mr. Hall made thirteen series of observations, each of forty readings, with the magnet excited, inactive, and reversed, successively. The results were all but negative. It was not, however, completely shown that a magnet does not tend unsuccessfully to deflect a current. To test this point, a disc or strip of metal, forming part of an electric circuit, was placed between the poles of an electro-magnet, cutting across the lines of force. The two poles of a sensitive galvanometer were then placed in connexion with different parts of the disc, until two nearly equipotential points were found. The magnet current was then turned on, and the galvanometer was observed without result. The experiment was repeated, using gold-leaf mounted on a plate of glass or a metal strip. A decided deflection of the galvanometer was thus obtained. It was permanent, and therefore not accounted for by induction. It was reversed when the magnet was reversed; but not by transferring the poles of the galvanometer on the strip. It was just what would be expected if the current were pressed, but not moved, towards one side of the conductor. In regard to direction, if we suppose the current a single stream, flowing from positive to negative poles, *i. e.*, from carbon to zinc, the phenomena indicate that two parallel currents in the same direction tend to repel each other. By the opposite supposition they attract. It is well known that two conductors bearing parallel currents in the same direction, are drawn towards each other. It is early to decide whether these facts have any bearing on the absolute direction of the current. Quantitative determinations on the subject were also commenced. 'It is, perhaps, allowable,' says the writer, 'to speak of the action of the magnet, as setting up in the strip of gold-leaf a new electromotive force, at right angles to the primary force.'

Hall's Discovery of a New Action of Maynetism on Electric Currents is stated by Professor Rowland as follows:—Whenever a substance transmitting an electric current is placed in a magnetic field, besides the ordinary electromotive force in the medium, we have another acting at right angles to the current, and to the magnetic lines of force. Whether there may not be also an electromotive force in the direction of the current, has not yet been determined with accuracy; but it has been proved, within the limits of experiment, that no electromotive force exists in the direction of the lines of magnetic force. This electromotive force in a given medium, is proportional to the strength of the current and to magnetic intensity, being reversed when either of these is reversed. It has also been found to differ in direction in iron from that in gold or silver. In gold the effects are such as would happen were the electric current to be rotated in a fixed direction with respect to the lines of magnetic force, to an amount depending only on the magnetic force, and not on the current. This fact seems to point to another very important case of rotation, namely, that of the plane of polarized light. By Maxwell's theory, light is an electrical phenomenon, and consists of waves of electrical displacement, the currents of displacement being at right angles to the direction of propagation. If the action takes place in dielectrics, the rotation of the plane of polarization of light is explained.

After working out the complete theory as to light, on the assumption that the displacement currents are rotated as well as the conducted, he finds the result very satisfactory. He thinks there are strong grounds for supposing the two phenomena to be the same.

A new form of Siren is described by Lord Rayleigh in the *Philosophical Magazine*. Some years ago he observed that a light pivoted blade is set in rapid rotation when exposed to wind. The phenomenon is of the same character as the rotation of a slip of paper falling freely in air, which was discussed in 1854 by Professor Maxwell. In both cases, the rotation may occur in either direction, proving that its cause is not to be looked for in any want of symmetry. The present is an application of a principle, the explanation of which he thinks has yet to be discovered. A blade is cut out of sheet brass, and provided with sharp projecting points, bearing in hollows at the end of two set screws, adjusted till the blade can turn in a small wooden frame freely, but without shake. Pieces of cardboard, or metal, are made to fit the blade pretty closely, so that when all are in one plane, the aperture is almost closed. The blade, on turning, acts as a revolving stopcock. He has made several sirens on this plan, which perform well. If the wind from the bellows is admitted symmetrically, they will revolve in either direction, and soon acquire sufficient speed to give a note of moderate pitch. The position of maximum obstruction is, for small displacements, one of stable equilibrium. If a larger displacement is made, the vibration tends of itself to increase up to a certain point, or even to pass into continuous rotation; but the precise behaviour in this respect probably depends on the details of construction.

In the same paper he also describes experiments for demonstrating—

The Acoustical Shadow of a Circular Disk.—In Poisson's experiment, a bright point is observed in the centre of the shadow of a circular disk, on which waves of light are directly incident. To obtain the acoustic analogue of this phenomenon, it is advisable to use sounds of very high pitch, which have the advantage of readily exciting sensitive flames. The best results were obtained with a squeaky toy-reed. A bird-call was blown, with a pressure of four inches of water, and placed about twenty inches from a disk of fifteen inches diameter. The observation was made at a distance of twenty-four inches on the other side of the disk, and succeeded, both with the ear and with a sensitive flame. In the former case a plate of wood, bored with a hole about $\frac{1}{4}$-in. diameter, was held against the side of the head so that the hole was opposite the ear-passage. The head was moved until the position of maximum sound was determined. To verify the fact that the position of maximum sound was really at the centre of the shadow, a hole bored through the centre of the disk was closed with a cork during observation. This was afterwards removed, and then the eye could see the source of sound through the two holes. The most suitable flame was that from a pin-hole burner, brought near to flaring-point by gas pressure of about ten inches. In a subsequent experiment, a toy-reed was substituted for the bird-call, and answered better with the flame than with the ear.

The Electromotive Forces in free jets of Water have been studied by Julius Elster, under the guidance of Professor Quincke. He claims as results:—

1. For E. M. F. to arise in a free jet, the water must be in contact with a solid body.

2. E. M. F. is only evolved where the particles of fluid undergo friction, so that only a small part of the jet contributes to its development.

3. When the velocity of the jet is altered, the E. M. F. is proportional to the *vis viva* of the particles.

4. The E. M. F. varies with the nature of the bodies in contact.

From which observations he concludes: That (α) motion of a fluid by itself does not produce E. M. F. (β) Capillary currents are conditioned solely by friction of particles; in those which do not wet the wall of the tube, by friction against this; in those which do wet it, by friction against a layer of fluid condensed against the wall. (γ) Capillary currents are identical with the friction currents appearing in the rubber of an electrical machine.

The Acceleration of Gravity for Tokio, Japan, formed the subject of a paper read before the Physical Society by Messrs. Ayrton and Perry. They proceeded by the following method:—A brass ball, 2352·2 grammes in weight, was suspended by a long steel wire 0·45 millimetre in thickness, and in the earlier experiments 978·7 centimetres in length. The wire was supported from a steel knife-edge resting on a brass plate. Both the brass ball and the bob of the seconds-pendulum of the standard clock were fitted with fine pieces of platinum wire, either of which dipped into a small cup of mercury when the pendulum to which it was attached was vertical. The mercury-cups, &c., were then joined up with a battery and resistance-coils to a quick-running Morse instrument. The whole constituted what is known as a 'break-circuit chronograph,' that is, a continuous ink-mark was made on the paper run out by clockwork, broken by a very small gap each time the wire attached to the bob of the seconds-pendulum passed through the mercury. These breaks, then, in the ink line indicated seconds; if, however, both pendulums were simultaneously in the vertical line, no break was made. Hence the absence of a break in the line at the end of any special second indicated coincidence of the two pendulums; and in this way the times of a large number of coincidences could be automatically registered.

During this set of experiments they could not measure the long fine steel wire with as much accuracy as was desired, since, although they had two or three brass scales, the makers had omitted to record on them at what temperature they were correct. However, assuming that one of them was accurate at 0° C., then a rather large number of experiments gave, as the value of g, 978·8 centimetres per second as a first rough approximation.

Subsequently they obtained from the Finance Department of Japan the loan of two very beautiful brass scales, by Deleuil of Paris, and guaranteed correct at 0° C. One was graduated in millimetres; the other consisted of a brass rod, with two pieces at its ends at right angles to the rod, and the distance between the two planes of the inner surfaces of the pieces was exactly a metre at 0° C. They had, then, the means of making a far more complete series of experiments than before; but as their trial pendulum was nearly ten times as long as the seconds-pendulum of their clock, the method of coincidences was an inconvenient one; and so they merely adopted the following:—The long pendulum alone controlled the 'break-circuit chronograph;' so that the number of breaks in the line during any time

indicated the number of vibrations of the long pendulum in that time. At the commencement of the experiment, after the pendulum had been set swinging, and the paper was running out at a fairly uniform speed, a mark was made on it by tapping sharply the armature up with the finger when a chronometer, lying beside the Morse instrument, indicated a certain time; and after an hour or so, the paper being kept running all the time, a second mark was sharply made on the paper when the chronometer indicated a certain other noted time. So much paper had then run out in the interval of time shown by the chronometer; and the breaks in the line, counted carefully afterwards by two independent students, gave the whole number of vibrations of the pendulum in that time. The fraction of a vibration could also, of course, be ascertained by comparing with the length of the first line made after the first break had been produced, on tapping the armature, and repeating the same process at the end of the paper. The experiment is independent of the rate at which the paper runs out, provided, of course, it is never allowed to run so slowly that there is any difficulty in distinguishing the different breaks electrically made by the long vibrating pendulum. The mean temperature of the wire was carefully taken at each experiment.

The next point was to measure accurately the length of the wire. As it was impossible to do this satisfactorily with the wire hanging up, it was taken down without disconnecting either the knife-edge carrying it or the ball at the other end. The knife-edge was then fixed at one end of a horizontal rail, and the other end of the wire close to the ball hung over a wheel with very little friction. By this arrangement the wire in a horizontal position was, of course, stretched as much as it was in the vertical position, as far as the effect of the weight of the ball was concerned. A correction had, however, to be made for the weight of the wire itself, which, of course, caused the tension to be a little less at the bottom than at the top when the pendulum was hanging up vertically. A few centimetres of similar fine steel wire being weighed, a simple integration gave the small additional weight necessary to be added. This being done, the final result obtained was that the length of the pendulum equalled 939·09 centimetres at 0° C.; and the consequent value of g in air for Tokio, Japan, calculated from the result of about eighty thousand vibrations of the long pendulum, would be 980·06 centimetres per second per second, if the pendulum could be regarded as a simple mathematical pendulum.

Correcting Factors.—1. The two most obvious corrections to apply to this result are the corrections for infinitely small arcs, and for the air-friction —neither of which was found of any practical consequence, on account of the very small angle through which the pendulum usually swung, and that the decrement of the amplitude of the vibrations was imperceptible, even after many swings. Although, however, such a pendulum as they were using approaches very nearly a perfect simple pendulum, there are certain causes of possible error arising from its flexibility and slight elasticity which would not effect a rigid compound pendulum. To estimate the practical effect of these possible errors, it is necessary to solve generally the complete problem of a heavy ball supported by an elastic wire, one end of which is soldered to the ball and the other end to a steel knife-edge. When a sus-

pended ball is swinging in the arc of a circle, we know that near the end of a swing the attachments of the ball have to resist a tendency for the ball to turn. For since the ball has been turned in passing from its lowest to its highest position, it would continue to turn were it not stopped by the wire itself. At the end of every swing, then, there must be a slight kick; so that in fact the ball will make minor swings about its point of attachment all the time of the motion. To make this kick less perceptible, we must make the fastening of the wire to the ball capable of resisting the tendency of the ball to continue its turning motion. If we do this by soldering the wire, a smaller kick will result, and will be due to the bending-moment of the wire resisting the turning action. If there were no difficulty of construction, it might be better to get rid of this kick difficulty by making the bob capable of rotating in the plane of swinging about an axis through its centre of gravity.

2. Next, with regard to the stretching of the wire arising from variations in the centrifugal force of the ball while swinging. Since the time of a complete vibration of their pendulum was nearly 6 seconds and the arc about 30 centimetres, the velocity of the middle of its path was 15·7 centimetres per second; hence the pull on the wire, which at the end of the swing was equal to the weight or the wire, or 2352·2 grammes, was increased by less than a gramme, so that no practical extension of the wire arose from centrifugal force.

3. Shortening of the length of the wire, due to its curvature, arising from the resistance of the air making it concave in the direction of motion. It is easy to see that the shortening of the pendulum due to this cause is excessively small, and is of the same order as the lengthening arising from the centrifugal force; so that these two very small errors may be regarded as balancing one another.

Also, since it may be calculated that the period of transverse vibration of the wire is less than one-fortieth of the periodic time of the pendulum, the resistance of the air cannot tend to cause amplification of the lateral vibrations in the wire itself.

It may, therefore, be assumed that the pendulum vibrated like a rigid body, consisting of a ball of brass, a straight steel wire, and a triangular steel prism, of which the edge was the fixed axis.

The steel knife-edge had a length of about 4 centimetres, a breadth of about 1 centimetre, and a depth of $\frac{1}{2}$ a centimetre; hence its weight was about 7·8 grammes, its moment of inertia about the axis of rotation 0·98 (gramme, centimetre), and the distance of its centre of gravity from the axis of rotation 0·33 centimetre. The weight of the wire was 11·6 grammes, and its length 934·99 centimetres at 0° C. Its moment of inertia was therefore $3{\cdot}3803 + 10^6$ (gramme, centimetre), and the distance of its centre of gravity from the axis of rotation 467·49 centimetres. The weight of the brass ball was 2352·2 grammes, its moment of inertia about the axis of rotation $2{\cdot}0744 + 10^9$, and the distance of its centre of gravity 939·09 centimetres at 0° C. Of the whole system, then, the weight was 2371·6 grammes, the moment of inertia about the axis of rotation $2{\cdot}0778 + 10^9$ (gramme, centimetre), and the distance of its centre of gravity $2{\cdot}2144 + 10^6$. Consequently

$t = 3{\cdot}0748$ seconds;

or

$g = 979{\cdot}58$ centimetres per second per second in air,

or

$g = 979{\cdot}74$ *centimetres per second per second* in vacuo *for the Imperial College of Engineering, Japan*—

a result agreeing extremely closely with the number 979·7 obtained from Clairault's formula.

In beginning this series of observations it was expected to find g to be greater than Clairault's formula gives it. Clairault's formula assumes a circular equator; Capt. Clarke has found that the equator is elliptical, one extremity of its major axis being in 15° 34′ E. longitude; and therefore Tokio is in longitude nearer a minor axis than a major one. There may be, however, a reason why g satisfies so well Clairault's formula, in spite of this excentricity of the equator. The greatest depression of the Earth's surface is only a few hundred miles to the east of Japan; and probably the diminution in g produced by this cause just counter-balances the increase of g produced by ellipticity of the equator. As for local perturbations, it is to be remarked that Tokio is situated on a very large plain, there being no hills of any magnitude within eighty miles. The geodesy of Japan is of special interest on account of the great Pacific depression, and on account of the very gradual slope of the earth's surface from Japan to China, which causes Japan to be a sort of ridge.

On the Dynamo-electric Current is the title of a paper read before the Royal Society by M. Siemens. He points out that the Gramme and Siemens machines are both subject to the drawback that an increase of external resistance causes a falling-off of the current, and that, on the other hand, short-circuiting the outer resistance, through contact between the carbons of the lamp, much increases the electric excitement of the machine, and the power necessary to maintain its motion, giving rise to rapid heating, and to destructive sparks in the machine itself. An observation in a paper of Wheatstone's is referred to, showing that a powerful current is set up in the shunt-circuit of a dynamo-electric machine, which has been since taken advantage of in the Ladd and Brush machines as a current-generator.

The chief object of this paper is to show how machines worked on the shunt system can be made to give maximum results. It seems that the resistance on the rotating helix has to be greatly reduced, by increasing the thickness of wire employed, and that on the magnets increased more than tenfold, by increasing the length and weight of coil-wire employed. The results are thus summarized,—

1. The E.M.F. increases at first rapidly with increased resistance, and then more slowly towards an asymptote.

2. The current in the outer circuit is actually greater for a unit and a-half resistance than for one unit.

3. With external resistance of one unit, about equivalent to an arc through which 30 to 40 Webers are passing, 2·44 horse-power is expended, of which 1·29 is useful, giving an efficiency of 53 per cent as compared with 45 per cent of the ordinary machine.

4. The maximum energy which can be demanded from the engine

is 2·6 horse-power, so that only a small margin is needed to suffice for the greatest demand.

5. The maximum energy which can be injuriously transformed into heat in the machine itself is 1·3 horse-power, so that there is no fear of destroying insulation in the helix by excessive heating.

6. The approximately maximum current is habitually used, so that the commutator and collecting-brushes are quite capable of transmitting it.

The new machine gives steadier light, with greater economy of power, is less liable to derangement, and may be driven without change of speed by a smaller engine, and is free from all objection when used for electro-deposition.

It enables the author to effect an important simplification of the lamp regulator, dispensing with all wheel and clockwork.

A New Zinc-Carbon Battery, patented by Mr. R. Anderson, is excited by means of hydrochloric acid, bichromate of potash, and certain other 'salts.' It may be used either with or without a porous pot. It is stated to have an E.M.F. of 2·15 Volts, to be free from local action, from internal resistance, and to be very constant.

Toughened Glass appears to be less easily penetrated by the spark of the induction coil than that of the ordinary description. Ducretet, to whom the observation is due, proposes to utilize it for the production of Leyden jars and condensers.

Rotation under the Earth's Magnetic Influence is found by Sig. Agostini to occur in a drop of mercury when an electrical current is sent vertically through it. If such a drop be placed on the pole of a steel magnet, to which one terminal of a weak battery is attached, and the other terminal introduced into the drop, it also undergoes rotation. In this manner the distribution of magnetism in bars, and the neutral points, may be studied. The motion is rendered visible by strewing lycopodium on the mercury.

A New Galvanometer has been brought before the Physical Society of Paris by M. Marcel Deprez. It consists of a series of soft iron needles suspended between the limbs of a steel horse-shoe magnet of great power. Parallel to these needles are wound a few coils of stout wire to conduct the current. It is adapted to measure currents of considerable strength.

Influence of Heat on Tuning-forks has been measured by Rudolph König. He finds that up to 50° or 60° C. it is practically constant. Thick tuning-forks are more influenced than thin ones of the same pitch, showing that change of elasticity, and not change of length in the prongs, is the efficient cause of the alteration. The influence on forks of different pitch is proportional to their vibration-numbers. Generally the period is changed $\frac{1}{8943}$ by a difference of 1° Centig. The change in pitch of the normal $C_3 = 512$ vibrations per second at 20° for 1° Centig. is 0·0572 vibration. König has constructed a compensated fork, which at any temperature gives exactly 512 vibrations.

Variations from Mariotte's Law have for some years occupied the attention of Mons. Amagat, who has succeeded in making exact measurements of the changes in volume of gases when submitted to the pressure of a column of mercury over one-fifth of a mile in height. He has committed the results to the *Annales de Chemie et de Physique*. The place chosen for the

experiment was the coal-mine of Verpilleux, near St. Etienne. This is 327 metres, or about 408 yards deep. The temperature at the bottom is very constant. The method of experimenting consists in introducing a tube containing pure gas, such as nitrogen, into the large cavity of the apparatus, breaking off its point below the surface of the mercury with which the cavity is filled, and screwing the vessel down. Steel tubing, in sections of 2 millim. bore, is then screwed on to any required length, more mercury being forced into the reservoir by means of a powerful force-pump, until it mounts to the top of the tube. At each increase of compression the height of the mercury-column is measured, and the volume of the compressed gas is read off by means of a cathetometer. With nitrogen, compressibility slowly increases to a maximum at 65 atmospheres, decreasing again slowly to a normal figure at 91, then decreasing rapidly until at 430 atmospheres the volume is four-fifths of that given by Marriotte's law. Having obtained a nitrogen standard, other gases could be compared more easily with this than examined individually. Tables up to 400 atmospheres were obtained for air, oxygen, hydrogen, carbonic oxide, ethylene, and marsh gas. Graphic representations were prepared, the abscissæ representing pressures in metres of mercury, the ordinates the difference between the products of the pressures into the volume and unity, *i.e.* to the variation from Mariotte's law. The most conspicuous variations occur in the case of gases near liquefaction. Hydrogen is the only gas not exhibiting a minimum of product of pressure and volume. It seems probable that other gases, if forced to assume a degree of tenuity similar to that of hydrogen by means, for instance, of elevated temperature, would yield curves more and more resembling those which it furnishes. W. H. Stone.

ZOOLOGY.

The Systematic position of the Sponges.—Dr. Conrad Keller supports the notion originally put forward by Leuckart, and further developed by Häckel, that the Sponges are truly Cœlenterate animals, and not Protozoa. At the meeting of the Swiss Society of Natural Sciences in August last, he stated that at Naples, in the spring of 1879, he had the opportunity of closely observing the development of a siliceous sponge, which he named *Chalinula fertilis*, that he ascertained the existence in this sponge of separate sexes, and that during the period of reproduction the female presents a nuptial dress, her colour varying from carmine-red to blue. The ovum undergoes complete but irregular segmentation, resulting in the formation of a larva, composed at first of two, and afterwards of three lamellæ. He traced the transformation of this larva into a young sponge, and, according to him, it gave origin to a form which, with the exception of the absence of tentacles, agreed in all essentials with a young polype. He considered that his observations, in illustration of which he exhibited drawings, prove beyond doubt that the true position of the Sponges is among the Cœlenterata, of which he would make of them a third natural division (*Spongozoa*). (*Bibl. Univ.*, December 15, 1879.)

New Classification of Crustacea.—Dr. A. S. Packard, jun., has issued a sketch of his new scheme of classification for the Crustacea. He remarks that recent researches upon the embryology of the King Crab (*Limulus*), have shown certain most unexpected resemblances to the mode of development of the Arachnida;* but these are also met with in certain Crabs and Shrimps whose development is exceptional, so that the views of some naturalists, such as E. Van Beneden and Dohrn, that the King Crab is not a true Crustacean but an Arachnid, or the next thing to it, cannot be adopted. *Limulus*, according to Dr. Packard, must be regarded as a generalized or synthetic type combining with features of its own certain resemblances to the Arachnida and the normal Crustacea. In its mode of respiration, its external gills, and its circulatory organs, it is essentially a Crustacean; but it must be separated from the normal Crustacea, and form the living representative of a sub-class equivalent to all the other living Crustacea. The fossil Merostomata (*Eurypterus*, *Pterygotus*, &c.), are closely allied to *Limulus*, and Dr. Packard considers the Trilobites to be nearly related to the Merostomata. For his new sub-class he proposes the name of *Palæocarida*, most of its representatives being old fossils; the normal Crustacea form his sub-class *Neocarida*.

Dr. Packard gives the following table to show the mode of arrangement of the different orders of Crustacea under these two sub-classes:—

Classification of the sub-classes and orders of Crustacea.

CRUSTACEA.	NEOCARIDA.	Decapoda.
		Stomapoda.
		Tetradecapoda.
		Phyllocarida.
		Branchiopoda.
		Entomostraca.
		Cirripedia.
	PALÆOCARIDA.	Trilobita.
		Merostomata (King Crabs, &c.)

The PALÆOCARIDA show the following characters:—Appendages of the cephalothorax in the form of legs rather than jaws; no antennæ; brain on the same plane as the cephalothoracic ganglionic ring and supplying nerves to the eyes alone; nerves to the cephalothoracic appendages sent off from an œsophageal ring; nervous system ensheathed by a ventral system of arteries; metamorphosis slight; sexes distinct.

Order I. MEROSTOMATA.—No distinct thoracic segments and appendages. (*Limulus*, *Eurypterus*, &c.)

Order II. TRILOBITA.—Numerous free thoracic segments and jointed appendages. (Trilobites, all extinct.)—*American Naturalist*, Dec. 1879.)

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* We may remark upon an interesting piece of evidence in favour of the relationship between the Arachnida and *Limulus*, furnished by Dr. J. Barrois' researches on the embryology of the Spiders. Dr. Barrois denominates an important stage in the development of Spiders 'the limuloid stage,' the embryo at this point having a close resemblance to the King Crabs.—See *Ann. and Mag. Nat. Hist.*, March 1880.

Amœba Blattæ.—Prof. Leidy brought under the notice of the Academy of Natural Sciences of Philadelphia the occurrence in the interior of the cockroach of an amœbiform parasite described by Prof. Bütschli under the name of *Amœba Blattæ*. He found it abundantly associated with two other Protozoans and two parasitic worms of the genus *Oxyuris*. Prof. Leidy thought that this amœboid parasite differed sufficiently from *Amœba* to be placed in a distinct genus, which he names *Endamœba* in allusion to its occurrence as an internal parasite; it differs from *Amœba* in the absence of a contractile vesicle, and commonly also of vacuoles, and in the want of differentiation of endosare and ectosare; and from *Protamœba* in the possession of a well-defined nucleus. Prof. Leidy adds:—'*Endamœba Blattæ* affords a good example of a primitive, active *nucleated organic corpuscle*, or a so-called *organic cell without a cell-wall*. In the encysted condition it would be a complete nucleated organic cell. *Endamœba* may be recommended as a convenient illustration of a primitive form of the organic cell on account of its comparatively ready access.' He gives as the size of globular forms, 0·054—0·075 millim. in diameter and of elongated forms 0·075 by 00·6 millim. to 0·15 by 0·09 millim.—(*Proc. Acad. Nat. Sci. Philad., October* 7, 1879.)

A Freshwater Medusa.—A fortnight ago zoologists were startled by a report that a small Medusa had been found living in great abundance in a tank containing fresh water in the Botanic Gardens in the Regent's Park. The tank was that in which the *Victoria regia* is grown, and the water in it is habitually kept at a temperature of 85–90° F. The case was particularly remarkable, for no known example of a Medusoid organism living in fresh water was previously known—in fact, contact with fresh water is generally fatal to such animals—and although they may be found in the mouths of rivers, it is always in that part of the tideway where the water is quite salt that they are met with. The little Medusa belongs to the so-called naked-eyed division of the late Prof. Forbes. The general run of the specimens are about the size of half a pea, but some of them are said to attain a diameter of about half an inch. They consist of a nearly hemispherical bell, from the centre of which depends a long stomachal peduncle, hanging down some distance below the margin of the bell. The bell or umbrella is traversed by four radiating canals, which start from the point of insertion of the stomachal peduncle and run to the margin of the bell. From these canals the generative organs depend, forming oval sacs between the two membranes forming the bell. These sacs contain either ova or spermatozoids, so that the animals are unisexual. The aperture of the bell, as in all naked-eyed Medusæ, is narrowed by a membrane (velum), extending inwards from the margin of the bell, which bears a great number of tentacles, four of which are considerably larger than the rest, and correspond to the radial canals. The other tentacles are much more numerous. In large specimens, according to Prof. Lankester, there are seven secondary tentacles in each of the spaces between two primary tentacles, while the interspaces between the secondary tentacles are occupied by groups of six tertiary tentacles. This gives 224 as the total number of tentacles on a fully developed specimen. The otoliths are placed along the line of attachment of the velum, and are about 80 in number; the otocysts, according to Prof. Lankester, are produced

in the form of canals into the velum, a peculiarity which he thinks will necessitate the formation of a new family or sub-order for this animal.

The little freshwater Medusa from the Victoria tank, has been described by Prof. Allman and Prof. Lankester, and has the questionable privilege of already possessing three names. Prof. Allman named it *Limnocodium Victoria*, in a paper read before the Linnean Society; Prof. Lankester in *Nature* gave it the denomination of *Craspedacusta Sowerbii*, but changed the generic name to *Periplanella*, in a communication read to the Royal Society. Nothing is known as to the means of its introduction into the tank in which it was found: this has been used for years for the cultivation of the Victoria Water-Lily, which is an annual grown from seed; the seed used is ripened in this country; and the tank is empty and dry for several months every year. Of course nothing is known of its life-history, except that it was observed by Mr. Sowerby feeding on *Daphniæ*. Prof. Lankester regards it as belonging to Häckel's order Trachomedusæ, and family Petasidæ, and as probably most nearly allied to the genus *Aglauropsis* of Fritz Müller, from the coast of Brazil. It is therefore probably one of those forms of Medusæ which have no fixed polyp-like stage in their development.

Prosopistoma.—M. Vayssière has continued his studies upon this curious insect (see *Pop. Sc. Rev.*, N. S., vol. ii. p. 444) which was supposed to be a permanent aquatic form of the Ephemeridæ. He has now observed its metamorphosis, and describes the results as follows:—Towards the end of May, the amber-yellow colour of some of the specimens which he was keeping in captivity became less bright, and he could soon see through the skin the first lineaments of the new individual. A few days afterwards (on the 3rd of June) the animals escaped from the pupal envelope in the same way as the ordinary Ephemeridæ. In the perfect state *Prosopistoma* closely resembles the well-known genus *Cœnis*, so that its right to enter into the Ephemerine family is established. Its last segment bears three rudimentary bristles, representing the natatory setæ which it possesses in its aquatic form. (*Compt. Rend.*, 7th June, 1880.)

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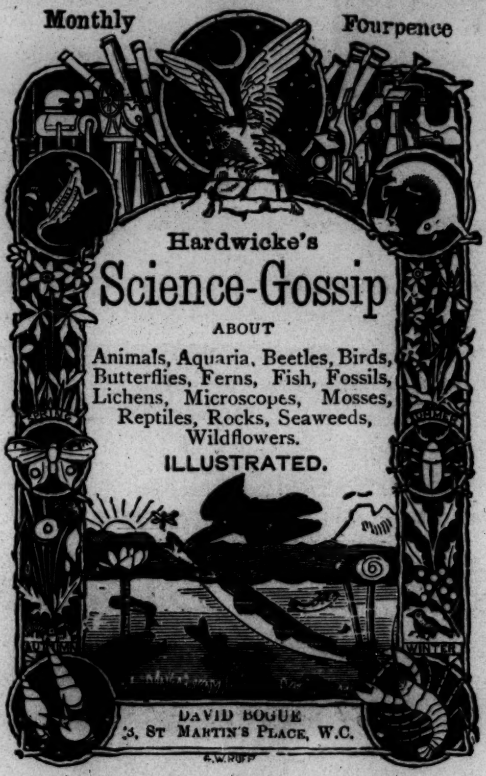

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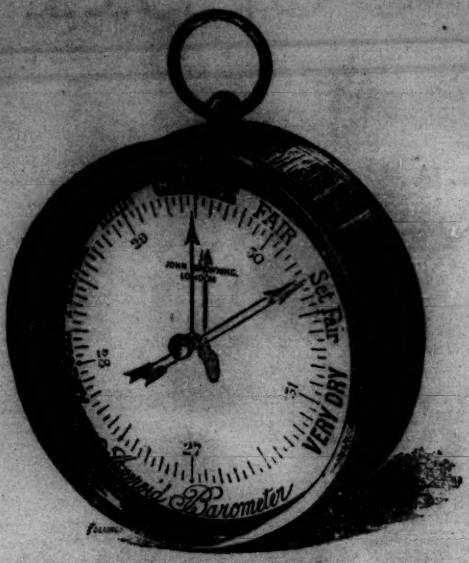

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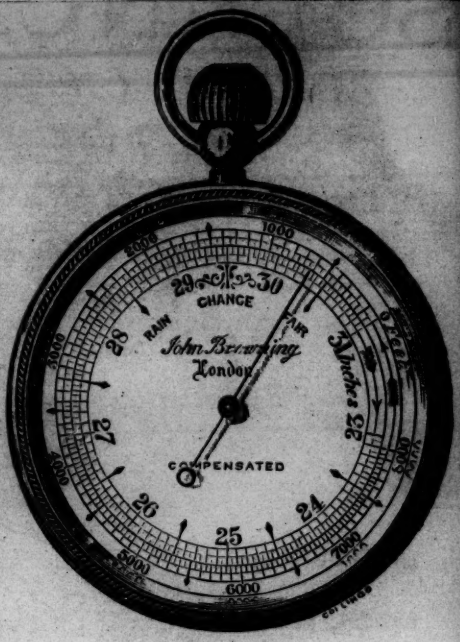

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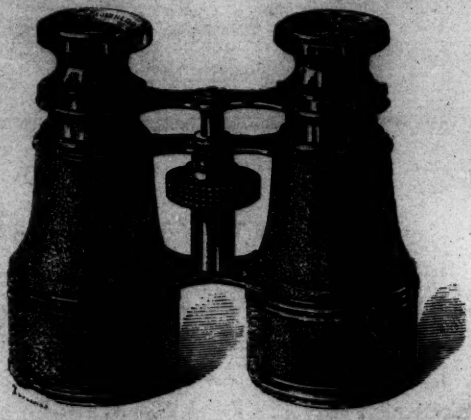

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